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THE BERTHON FOLDING CANOE.

CANOEING, under the auspices of Mr. Macgregor ("Rob Roy") is rapidly growing in popularity, and the members of the Canoe Club already number nearly 600. Under these circumstances we make no apology for occupying a good deal of space in illustrating a new and very ingenious canoe, which seems to supply a long-felt want. Many tourists have experienced the difficulty and annoyance of carrying a canoe about with them from lake to lake or river to river. That which we illustrate gets over the objection. The canoe has been designed by the Rev. E. L. Berthon on much the same principle as his well-known collapsing boats. Our engravings show the canoe opened and under sail; while below are a plan and cross sections of the boat open and folded up. In this condition it is but 3 in. thick, and its entire weight, with all fittings complete, is but 40 lb., so that it is not too much to say that one man can carry it with ease under his arm. The hull is made of canvas, double, with an air space between to secure buoyancy. The deck is of wood, and when forced down flat it is maintained in position by a couple of spring cross bars, as shown, detached or fixed in a moment, which effectively prevent the collapse of the boat. Besides these there are galvanized iron stanchions which prevent the floor from rising. The mechanical details have been very nicely worked out, and the result is a portable boat capable of carrying about 16 stones with perfect safety, and weighing itself, as we have said, but 40 lb. Our illustrations are so complete that we need only add that these canoes are built by the Berthon Boat Company, Romsey, Hampshire.—*Engineering.*

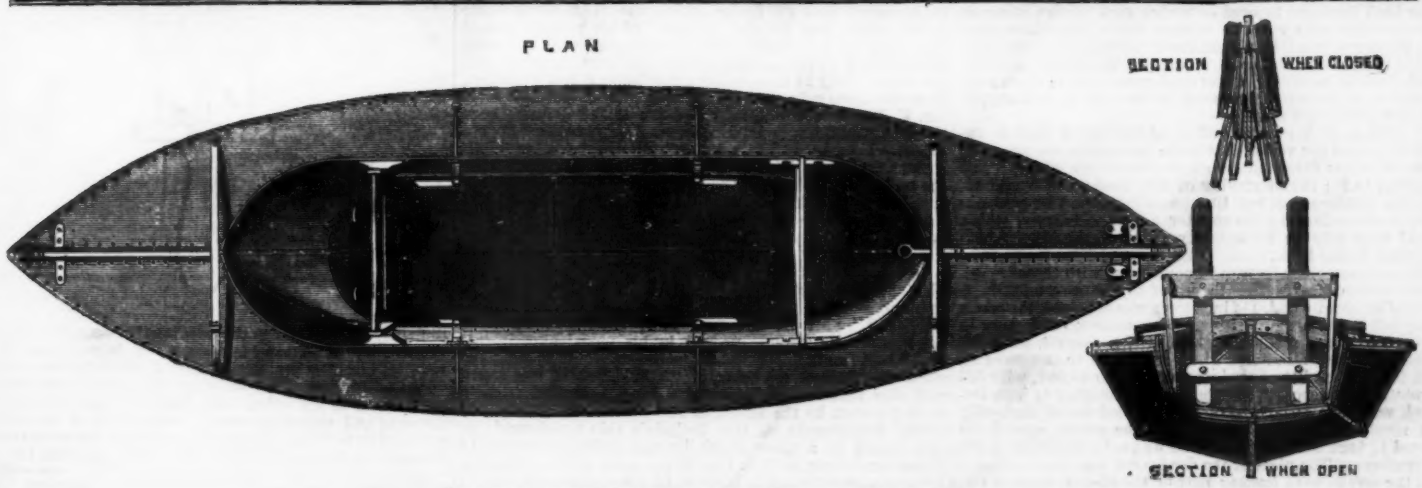
THE decline of the Clyde shipbuilding trade is exciting uneasiness. In 1874 the steam tonnage built on the Clyde amounted to 183,144 tons, the sailing tonnage to 69,166, or to 252,310 tons in all. Last year the Clyde turned out only 131,443 steam tons and 56,678 sailing tons; while the total tonnage built in the United Kingdom was 8 per cent greater than in 1874.

ON THE FORM OF THE STERN AND THE ARRANGEMENT OF THE PROPELLERS IN SCREW SHIPS IN ORDER TO OBTAIN THE BEST EFFECT IN PROPULSION.

By MR. ROBERT GRIFFITHS, C.E.*

IN the paper which I have the honor to read this evening, my motive is to draw attention to a subject in connection with screw propulsion which appears to have been overlooked and neglected by engineers and shipbuilders, viz., the form of the stern in screw ships which will leave the water in the best state for supplying the propellers, and the position in which the propeller should be placed in order to avoid the suction of the dead water, which is the principal cause of the increased resistance of the hull of screw ships. I am of opinion that this is the most important subject in connection with screw propulsion, being, if I may so term

* Read before the United Service Institution, April 21, 1879.



THE BERTHON FOLDING CANOE.

it, the very foundation on which the efficiency of the propeller depends. Since the introduction of the screw propeller the attention of engineers and shipbuilders has been chiefly taken up in improving the engines and the hulls of screw ships, and nearly all the improvements that have been made are due to the engineer, who has been able to obtain double the amount of power from the engines without increasing the consumption of coal; the shipbuilder, also, by lengthening the hull in proportion to the beam, has increased the carrying capacity of screw ships, without reducing their speed, and by fining the bow and stern, has reduced the resistance of the hulls; but as far as their sea-going qualities are concerned, there does not appear to be any improvement, for the frigates and ships of war, prior to the introduction of the screw propeller, were as good sea boats, if not better, than the modern built ships; and I feel convinced that that form of the stern would be best for screw propulsion which provides that the screw be placed as far back from the wedge end of the hull as is required to give time for the water to flow in at the stern to balance the head resistance.

The screw has not been improved as a propeller since 1800, for the screw then invented and patented by Shorter will give nearly, if not quite, as good results, as far as speed is concerned, as any that has since been invented; and all other qualities that are required are embodied in the screwI patented in 1849, and which has not yet been improved upon; in fact, the screw as a propeller has not been understood. How often have statements been published of trials made with new and improved screws which gave greater speed than Griffiths' screws, but which have never been heard of since; and what is the practice now? nineteen out of twenty ships are fitted with my propeller, the fact being that when a new screw is tried, it has either a different pitch, diameter, or position by which, in that particular case, some advantage in speed is obtained, and which is attributed to the propeller, until quite a different result is obtained with the next ship to which it is applied. It is now well known from trials which have been made that a few strips of plate iron, secured to a box on the screw shaft, and set at an angle which will hold the engines at the speed required, will give nearly as good results in speed as the best screw yet invented; it is therefore evident that we must look further than to alterations of the pitch, form, etc., of the screw, in order to improve upon the results which are at present obtained.

When making experiments in drawing water out of a model by the action of the screw I discovered that there was considerable suction of the eddy-water, caused by the screw at the upper part of its disk. I had a telescopic pipe, ¾ inch in diameter, which led from a tank inside the model, and which could be pushed out till the end was close to the top part of the screw disk. When this pipe was pushed out and was plugged at the inner end the model went 48 feet in a minute, with 700 revolutions of the screw; again, when the plug was removed from the pipe, and the water was allowed to flow into the tank, and be drawn out by the suction of the screw, the model went 61 feet with 700 revolutions, while, when the pipe was pushed in, the model went 75 feet with 745 revolutions. When the model was moored, the water would flow in through the pipe and fill the tank in 10½ seconds; if the screw was then started, it would empty the tank in 10 seconds, and when the model was going the screw would empty the tank in 8 seconds. These experiments were followed by others, by which I found that on the tapered part of the stern, near the screw, there was invariably a suction which materially increased the resistance of the hull, and I concluded that, instead of the taper of the stern coming close up to the screw, it ought to be finished some distance in front of it. In order to test this, I shortened the stern of the model and made the run as high up as the top of the screw, and 2½ inches in front of where it originally ended; from the end of the run to the sternpost I had a parallel piece of the thickness of the sternpost, and the screw was left in the original position; this, of course, made the run or wedge-form of the stern fuller. By this alteration the model gained 15 per cent. in speed, the power exerted being the same. I then altered the model to what it was before it was shortened, which reduced the speed to what it originally was.

I have since made experiments with four-bladed screws, with blades the same width from the boss to the points, and with blades tapered from the boss to the points on the leading edge only, the result being that by lengthening or fining the run there was a slight increase of speed of 3 to 5 per cent.; but by shortening the run and making it fuller, and placing the screw about two-thirds its diameter behind the tapered part, and leaving the intermediate space open, or filling in a parallel piece of the thickness of the sternpost, an increase of speed of 10 to 15 per cent. was obtained, for which I account in the following manner: As the ship moves forward the water closes in from each side of the stern to fill the space she occupied and balance the head resistance; when the screw is in operation it sucks away the eddy water at the top part of its disk, which mostly accumulates there, owing to the fullness of the run from the level of the screw shaft to the top of the screw, which water would follow the ship if the screw did not draw it away, by which the thrust given to the screw shaft is increased, and increases the resistance of the ship at least 40 or 50 per cent. more than it would have been had the screw not been in operation. The only remedy that I have been able to discover is to place the screw as far back from the tapered or wedge part of the stern as will allow the eddy water to remain there, for which purpose the screw must be placed about two-thirds its diameter behind the forward sternpost, so that the steam line currents which flow on each side of the dead-wood will be drawn by the action of the screw, and meet in front of it, and supply it with water to act upon.

In order to obtain the full effect of the power that is exerted to propel the vessel, it will be necessary to alter the sterns of screw ships (unless shipbuilders can give a valid objection to it); the shortening of the dead-wood would reduce the displacement but little, and that could be compensated for by enlarging the counter above the screw, which would then take in its wake a portion of the water which the screw forces back.

There is one other feature in connection with these experiments which proves the theory of my improvements. A piece of the dead-wood, 1½ inch long, was removed, the screw being left in the usual position. This made no difference in the speed, which was 51 feet in half a minute, with 525 revolutions per minute. The screw was then moved ¼ inch forward, and the speed was reduced to 46 feet, with 525 revolutions; the screw was then moved another ½ inch forward, when the speed was further reduced to 40 feet, with 520 revolutions; and, finally, when the screw was again moved ½ inch forward, the speed was only 26 feet, with 500 revolutions. The piece of the dead-wood was then replaced, and the screw, with leading part of the blades tapered off, was tried ½ inch from the sternpost, and gave a speed of 54 feet, with 545 revolutions; when tried 1 inch from the sternpost the speed increased to 58 feet, with 550 revolutions; at 1¼ inch it was 59 feet, with 550 revolutions, and at 2 inches from the sternpost 60 feet, with 555 revolutions.

There is very little doubt that in full stern and other ships the dead water that is formed between the stern and the place where the outside streams meet goes partly forward with the ship, and forms the wedge or taper-end to the dead-wood. This does not cause nearly as much resistance to sailing ships as it does to screw ships through the action of the screw sucking it away. Colonel Beaufoy, by his experiments (upward of half a century since), found that when the taper wedge end of his model was at an angle of 60° 22′ 45″, there was no resistance at any speed up to 8 knots; but when the angle was 19° 28′ 16″, the resistance at 8 knots was 34·34 lb., and gradually increased from 0·2101 lb. at a speed of 1 foot per second, to 34·34 lb. at 13·527 feet per second, his model being 12 inches square, with tapered wedge-ends.

Screw propellers, when working, act on the currents of water which flow by the stern of the vessel, and, by accelerating the velocity of those currents, meet with the resistance by which they are enabled to push the ship forward. There is, however, considerable difference in the velocity at which the currents flow at different parts of the screw's disk, and consequently the screw's blades meet with more resistance in passing over some parts of the disk than others. This, of course, results in the screw giving greater thrust in some positions, as shown in the dynamometer diagrams of Her Majesty's ship Rattler, in which the thrust varied between 2·9 and 4·1 turns in each revolution. Besides indirect proofs from thrust diagrams, direct proof of the difference of the velocities of the currents was obtained from experiments I was allowed to make with Her Majesty's steam pinnace No. 22, at Devonport, in 1875, by measuring, with apparatus specially constructed for the purpose, the rate at which they flowed through the screw's disk while the boat was towed. These experiments showed that over the bottom half of the screw's disk the water was little interfered with, and the water passed through the disk there at nearly the speed at which the boat was towed, but at the top half of the disk the water was dragged with the boat to a certain extent, and only flowed through the disk at about half the speed at which the boat was going. A diagram here shows the results, the currents having the least velocity where the shading is greatest.

When a screw is working, the blades as they pass over the top part of the disk meet with more resistance than when they are passing over the bottom part, for as the screw, to obtain the thrust, drives back a column of water the size of the screw-disk at a velocity corresponding to the power employed, it has to accelerate the velocity more at the top of the disk than at the bottom, and consequently more power is expended on the top part of the disk than on the bottom, and from this cause the increase in the resistance of the ship (which Mr. Froude has proved is always from 40 to 50 per cent. of the total resistance of the ship), is considerably more than if the power were uniformly expended over the disk, for the ship is much retarded by the drawing away of the dead water, so to speak, from the top of the disk, whereas accelerating the currents at the bottom of the disk would have hardly any effect on the ship. Again, as the blades one after another pass over the top part of the disk, and as at that place each blade meets with an increase of resistance, a series of jerks is given to the stern of the vessel, which is the cause of vibration.

I have here the diagram of a screw so constructed that the blades always meet with the same resistance. On the shaft is fixed a casting with two sockets on opposite sides; the shell of the boss, which can just pass over these sockets, has an elliptical hole in the forward end, through which the end of the casting passes, and which allows of a suitable amount of movement. The blades, which are formed so that most of their surface is aft of the middle of the boss, are constructed with flanges and shanks. These flanges are bolted in the usual way to the shell of the boss, the shanks slipping into the sockets; a cap completes the shell of the boss, and a cover fixed on the shaft in front of the boss prevents floating substances entering the boss through the elliptical hole. It is obvious that as the blades have more than half their surfaces aft of the middle of the shanks, the pressure of their surfaces tends to fine the pitch; and also, since they are rigidly connected with the shell of the boss, that, as one blade turns, the other is also turned, the pitch of one being increased as that of the other decreases. When, therefore, one blade meets with more resistance than the other, the difference of pressure causes that blade to turn, reducing its pitch and increasing the pitch of the other until the pressure on each is equal. By these means the injurious action of the screw is, to a great extent, done away with. The cost of construction of this screw would not exceed that of an ordinary screw, and no alteration in the shaft or vessel would be necessary for applying it, and at any time the blades could be fixed and the screw worked as an ordinary screw.

During the discussion of a paper on "Screw Propulsion," read at the meeting of the Institution of Naval Architects on the 3d inst., Mr. H. Samuel Mackenzie stated that, having seen in *Engineering* an account of some experiments with the screw moved close to the after sternpost, which I had made at the Royal Horticultural Gardens, he had the screw of his yacht moved back 9½ inches, which was all his screw frame would allow, and by this the speed of his yacht increased from 9¼ to 10½ knots; this corresponded with the extra speed gained by my model, but had there been room to move the screw about 24 inches (the screw being 5 feet diameter), he would have gained 2 knots at least. Another gentleman, Mr. T. Morrison Swan, informed me that, had there been time, he would have mentioned that a ship in which he was interested broke her screw shaft abroad; another shaft shorter than the original one was fitted, and in consequence the screw was brought 4½ inches nearer the forward end of the frame; this it was found caused a loss in speed of a knot, but on the return of the vessel to England the shaft was lengthened the 4½ inches, and the vessel returned to her original speed.

In conclusion, I beg to make a few remarks regarding the placing of the propeller as suggested. I find that it is the opinion of some people that my plans were invented and published by Mr. Froude some years since. There is no doubt that Mr. Froude proved by his model experiments that a saving of power would be effected by placing the screw one-third of the beam of the ship behind the stern, and also that the resistance of a ship is increased from 40 to 50 per cent. by the action of the screw; this he verified by his experiments on Her Majesty's ship Greyhound; he also stated in a paper which he read at the Institution of Naval Architects in 1874, that 58 per cent. of the indicated horse-power is wasted in friction of the engines and screw, and in the detrimental reaction of the propeller on the stream lines of the water closing in around the stern of the vessel. This I have no doubt is the case with screws placed near the forward end of the screw-frame at the termination of the wedge of the ship's stern, or with twin-screws when placed so that the points of the blades come close to the dead-wood; but if the screws are placed further off the dead-wood, and the stern of the ship is arranged as shown in my diagrams, they have the contrary effect, for a screw in that case, being two-thirds its diameter behind the tapered end of the ship, draws the stream lines toward it and feeds itself so that it leaves the eddy water to follow the ship in the space between the screw and the wedge end of the ship. With regard to placing the screw a distance behind the ship, Ericsson patented the plan in 1836, and afterward Beaty in 1850; it was also applied to the Frankfort merchant ship, and gave a very good result as far as speed was concerned, but the screw was moved to the ordinary position in front of the rudder in consequence of bad steering and the danger attending it in its unprotected position behind the rudder. It is a well-known fact to engineers and shipbuilders that there is no rule except the rule of thumb for placing the screw with regard to the lines of the ship and the power employed. This has been clearly shown by a ship recently built by first engineers and builders of the day, for at the first trials a speed two knots short of that which was expected was realized, the two knots being afterward got by only slight alteration in the screws, and no doubt most, if not all, of our naval and mercantile ships are from 10 to 20 per cent. short of the speed they would realize, if the screws were properly placed, or they might maintain their present speed with an equivalent saving of fuel.

USEFUL NOTES FOR MASTERS AND OFFICERS OF SCREW STEAMERS.

Q. DESCRIBE a right and left handed screw propeller.

A. A right handed screw propeller revolves from port to starboard over the top center, and a left-handed revolves from starboard to port.

Q. How is this knowledge essential in handling a screw steamer?

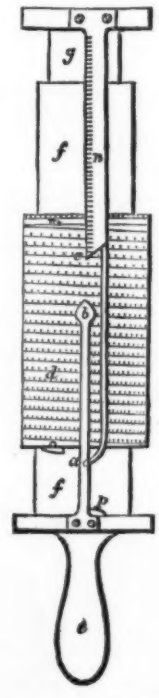
A. A right-handed propeller, going continuously ahead, will turn a vessel round faster under the starboard than under the port helm; but where she has to reverse and turn short round, she should be canted with her head to starboard. Backing the engines has a contrary and greater effect. Should the screw be a left-handed one the reverse will follow, viz., she will turn faster going continuously under a port helm than under a starboard helm; but should she have to reverse and turn short round she will cant with her head to port. A right-handed screw steamer, going ahead with her helm amidships, will revolve to port, but when going astern her head will fall to starboard, irrespective of the position of the rudder, and the reverse will occur with a left handed propeller.

Q. What does this teach you to guard against?

A. With a careless helmsman a screw steamer is apt to run to port or starboard of her course, as she has a right or left handed propeller, and as most vessels are fitted with right handed screws, and as the port helm is that in use when meeting end on or nearly end on, it should be used in ample time; also in crossing a vessel's track which is to starboard, should it be intended to pass under her stern. If, however, a vessel is near on the starboard bow, it would in most cases be preferable to starboard, knowing that the ship would answer her helm much quicker if she did not stop and reverse.—*Nautical Magazine.*

SPIRAL SLIDE RULE.*

THE method of multiplying and dividing by means of a rule was first introduced by Gunter about the year 1606 by the construction of a scale of two equal parts divided logarithmically, the readings being taken off with a pair of compasses. Oughtred, about 1630, invented the rule composed of two similar logarithmic scales sliding in contact, but the



difficulty of estimating the reading between two graduations then first became important. It is easy to see that it requires but little practice to place a graduation in one scale opposite to a position obtained by estimate between two graduations in the other scale, but it becomes a much more tiresome and uncertain process when both of the readings required to be placed in juxtaposition fall between two graduations on their respective scales. With practice, however,

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this operation can be effected with considerable accuracy provided the graduations are not too close together; hence to enable the calculations to be performed with a sufficient degree of approximation there has always been a desire to increase the scale and consequently the total length of the instrument. To attain this object and at the same time preserve the portable size of the instrument Prof. Everett designed his slide rule, but the range of this is now far surpassed by the invention of Prof. Fuller of the spiral slide rule.

The instrument can be readily understood from the accompanying figure.

d is a cylinder that can be moved up and down or turned round on the cylinder, *f*, *f*, attached to and held by the handle, *e*. Upon *d* is wound in a spiral a single logarithmic scale. Two other indices, *c* and *a*, whose distance apart is equal to the axial length of the spiral, are attached to the cylinder, *g*, which slides in *f*, and thus enables the operator to place them in any required position relative to *d*. *o* and *p* are two stops which, when placed in contact, bring the index, *b*, to the commencement of the scale. *m* and *n* are two scales, one attached to the movable indices and the other to the cylinder, *d*.

By the spiral arrangement the length of the scale can be made very great, and as only one scale is required the effective length is double that of an ordinary straight rule. The scale is made 500 inches, or 41 feet 8 inches long, and the instrument is thus equivalent to a straight rule 83 feet 4 inches long or a circular rule 13 feet 3 inches in diameter. The first three digits of a number are printed on the rule throughout the scale, much increasing the facility of reading off. The method of using the different indices will be best understood by examples. For multiplication—bring 100 to the fixed index, *b*, and place the movable index to the multiplicand, then move the cylinder so that the multiplier is at the fixed index. The product is read off at one of the movable indices, bearing in mind that the number of figures in the product is the algebraic sum of the number of figures in the multiplier and multiplicand, if it is not read upon the same index as the latter, but it is one less than that sum if read upon the same index. The use of the scales, *n* and *m*, is shown by the following: *n* being read from the lowest line of the top spiral and *m* from the vertical edge of the former. To find the value of 5^{13}: on placing *c* to 500, scale *n* reads ·68, and scale *m*, ·01897, which gives ·69897 for the logarithm of 5. ·69897 × 13 = 9·08661. Next placing the cylinder so that it reads ·08661 on scales *m* and *n*, the index, *c*, reads 12,207, hence the required power is 1220700000, consisting of ten figures as required by the logarithm above. Where a considerable degree of accuracy is required we believe this slide rule will be found of much service, but it cannot compete on the one hand, on account of its somewhat cumbrous nature, with the ordinary patterns of slide rule for rough and ready work, or on the other with a table of logarithms for calculations requiring close approximation.—*Nature.*

TYRE-GRINDING LATHE.

On the Austrian state railways considerable trouble has been experienced from the hardening of the surfaces of steel tires, due to the heating and subsequent sudden cooling caused by the application and release of brakes, this hardening being probably intensified by the use of steel tires containing a somewhat higher percentage of carbon than is usual elsewhere. The difficulty of dealing with these hardened tires by ordinary turning, led some time since—as it did many years ago at the Swindon Works of our Great Western Railway—to the adoption of a grinidng process, but the results were not satisfactory and it was abandoned. More recently, however, the arrangement which we illustrate for grinding these tires by emery wheels was designed, and it was set to work at the Simmering works of the company in 1876, since which time it has worked satisfactorily.

As will be seen from our engravings, the arrangement consists of an ordinary wheel-lathe fitted with two extra slide rests, on each of which an emery wheel is mounted. The emery wheels are driven by belts from an overhead shaft, and their spindles are set parallel to the surfaces of the tires, the slides which carry them being at the same inclination. By means of the arrangement of levers and links, shown clearly in Fig 3, an automatic alternative lateral traverse is given to the slides on which the emery wheels are mounted, so that the latter pass to and fro over the surface of the tire and operate on the whole width of the tread as the wheels rotate. As shown in Figs. 1 and 3, a slow inward feed is also given automatically.

Fig 2.

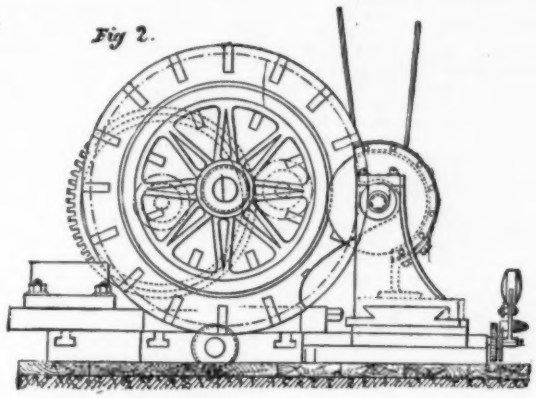


Fig. 1.

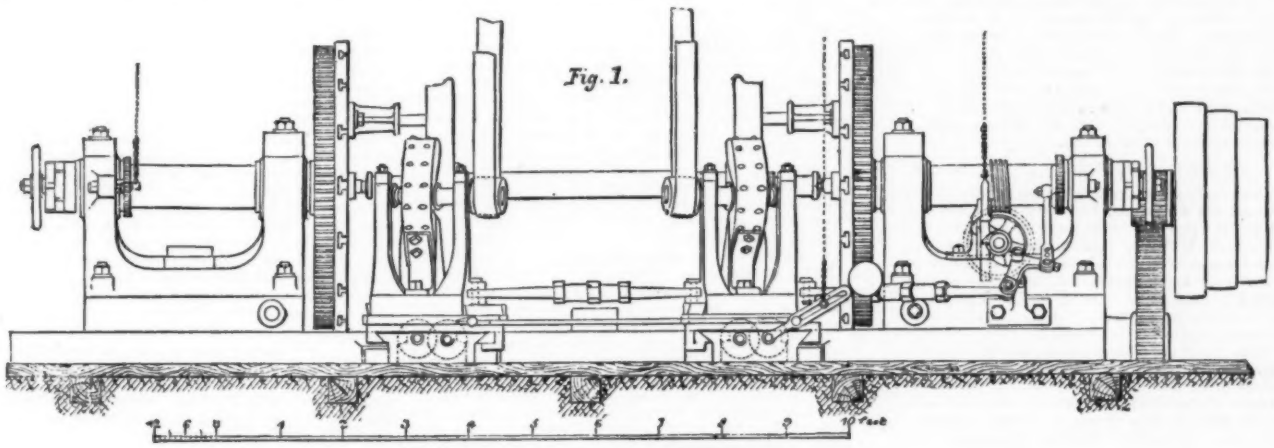
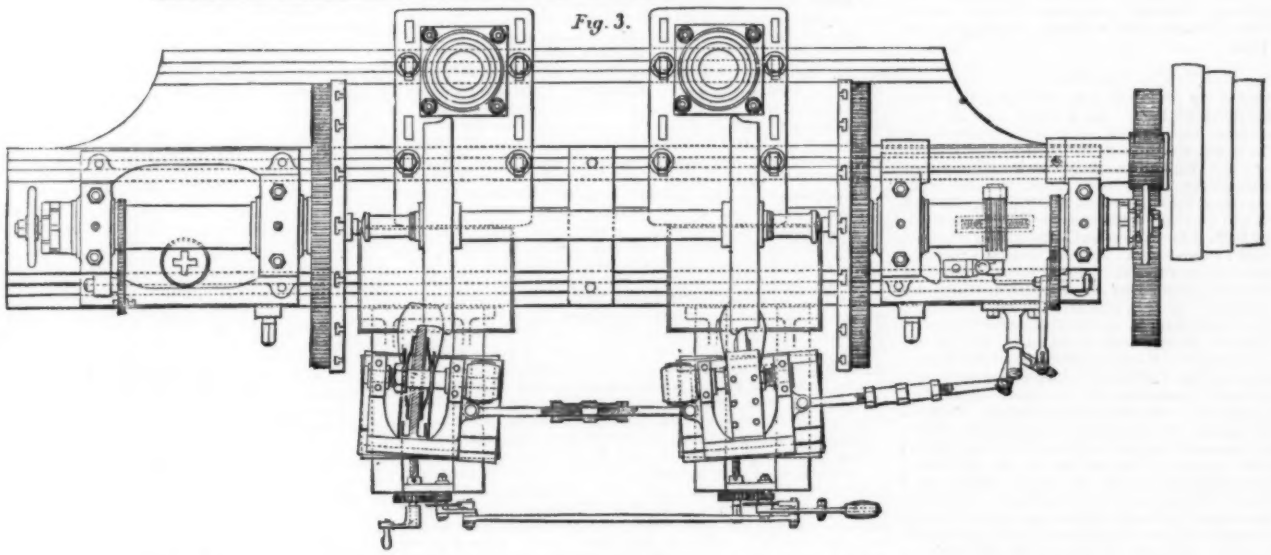


Fig. 3.



TIRE-GRINDING LATHE, AUSTRIAN STATE RAILWAYS, SIMMERING.

The wheels under treatment make one revolution per minute, and the emery wheels 800 revolutions per minute. The time occupied in re-dressing a pair of hard tires is stated to be two days three hours—rather long—and the cost is as follows:

	s.	d.
Labor	2	9·6
Renewal of emery wheels	0	3·12
General charges (40 per cent. of labor)...	1	1·44
	4	2·16

This cost appears rather heavy, but the company state that it is well compensated for by the saving in the quantity of material which it is necessary to take off by grinding to bring a tire to shape, there being no necessity to take a heavy cut to "get under the skin," as is the case when hard tires are turned in the ordinary way.—*Engineering.*

IRON AND STEEL.

AT the recent meeting of the Iron and Steel Institute, London, Mr. Edward Williams, the new president, delivered the following interesting inaugural address:

I must ask leave to begin my address to you with the expression of my grateful thanks for the high honor you have conferred upon me by placing me in this chair. To preside over the Institute is, of all others, the position I most esteem, and conscious as I must be that I cannot in many ways equal the able men who preceded me, I will give to the duties of the office my fullest and best attention. I hope that, by the good help of the members, the interests and usefulness of our valuable society will not be abated. It will be understood that in treating of steel I am not at all referring to the old material that is so well produced at Sheffield by the crucible processes. With this our Institute has not so far dealt much; and I am afraid that our good friends of the crucible trade look upon us as unwelcome intruders, bringing cheap steel that sooner or later may in some degree clash with their specialty. It is to steel in bulk for railway requirements and other constructive works, for ships, etc., that my remarks are intended to apply. For the production of such steel the world is indebted mainly to Mr. Bessemer, and in no small degree to our distinguished ex-president, Dr. Siemens, to both of whom Mr. Mushet's ingenious and most opportune invention is essential.

INITIAL PROCESSES.

For both wrought iron and steel the starting-point must be pig iron, which so far has not been produced at moderate cost except by means of the blast furnace. It is perhaps not wrong to hold that our best blast furnace plant and appliances are unlikely to be much improved. A greater volume of blast and more security for regular charging and working would probably be in most cases beneficial; but in the important item of fuel consumption, it is doubtful whether any large economy will be found practicable. The

waste gases of the furnace, which at present heat the blast and raise the necessary steam, are, no doubt, capable of doing more duty as fuel; but in works consisting of blast furnaces only, it is not easy to see how this can be availed of. Where, in addition to blast furnaces, there are wrought iron and steel works close at hand—a combination likely to become more prevalent—the more complete utilization of the gases will no doubt be taken advantage of. In Cleveland, where there is a great deposit of salt, it is reasonable to expect that some day brine will be evaporated on a large scale by means of the waste heat, an enormous quantity of which is now being thrown away into the air from boilers, hot-blast stoves, and the several kinds of furnaces.

THE BESSEMER DISCOVERY.

We many of us remember how we were startled when, in 1856, at the Cheltenham meeting of the British Association, Mr. Bessemer published his invention in detail. There was, among the prominent ironmasters of the time, pretty general doubt as to some of the principles he promulgated. In less than a week from the reading of the paper trial was made at Dowlais of the system of blowing air through pig iron, with complete success. In fact, an experiment undertaken to show that Mr. Bessemer had fallen into mistake proved the exact contrary. It has long been beyond dispute that no skill and care can so weld together the many pieces of iron constituting a rail-pile as to make it equal, or anything like equal, to the solid ingot for rails to withstand the tremendous strain of heavy railway traffic. It is strange, however, that, notwithstanding this, the inflation that, to our subsequent sorrow, came upon the iron trade after 1869, actually brought about a considerably increased make of built-up iron rails.

PUDDLING.

So late as 1875 our then President, Mr. Menelaus, who was, as he continues to be, in the forefront of the steel industry, gave in his address the chief place to puddling, and ironmasters generally looked anxiously for machinery to supersede hand labor. Now the case is very different. Puddling has been improved but little. The revolving furnace, upon which several years of skill and labor had been unsuccessfully expended in this country, was reported to have succeeded in America, and was from thence brought here in its most approved form. It must, however, be admitted that, notwithstanding very full trial, our hopes have been disappointed, and only very moderate advantage has accrued from the several machines introduced to lighten the work of the puddler. For rail-making, the puddling forge has disappeared, and it is highly improbable that it will return. The place of it has been taken almost entirely by the Bessemer pit, which, in principle, general arrangement, and almost minute details, remains as it came from the hands of the inventor, and seems scarcely to admit of much improvement. Even the ingot-mould, objectionable because of its inconvenient taper, has, so far, defeated all efforts to amend it, and continues in almost exclusive use.

IMPROVED WORKING.

Sir Joseph Whitworth, whose great skill and eminent services to engineering are notorious, has for a long time had in operation a system devised by himself of compressing fluid steel by enormous hydraulic power, and the advantage is said to be very great; but so far, I believe, the compressed steel has been but little used, except for armor to protect ships-of-war, and for the manufacture of guns. The open-hearth system, initiated, in this country at least, and so ably developed by our late president, Dr. Siemens (cheers), whose regenerative furnace is of the utmost value, has also extended considerably. For the very high classes of steel, where absolute soundness and great ductility are the first considerations, the open-hearth system is no doubt very efficient. The time necessary for the process allows of frequent testing, and there can be given to the metal exactly the composition desired. The Siemens-Martin process is much availed of for converting old iron rails into steel, or more correctly speaking, into ingot rails.

STEEL AND ITS USES.

It would be well if we could agree upon a fuller and more accurate nomenclature of iron. At present we call steel everything made from cast ingots, though in composition and character the metal varies from the hardest cast steel to the softest boiler-plate, which has qualities almost like copper, and should therefore, I venture to think, be more accurately designated. It is inevitable that new rails made from old iron ones should be high in phosphorus, but to compensate for this they have little carbon and silicon, and in actual work they are said to stand well. On the table is a piece of the first Bessemer rail ever rolled. It was made at Dowlais in 1858, from an ingot supplied by Mr. Bessemer, which, by the way, was parallel. A full analysis by Mr. Stead, of Middlesborough, is attached to the sample, which has, as will be seen, 0·446 of phosphorus, a quantity that would frighten us in these days; but carbon and silicon are almost absent. The ingots of which that just mentioned was one, were rough, and of most unpromising appearance generally, but they rolled perfectly. A second lot of ingots, received shortly afterwards, though similar in appearance, rolled very badly, only three or four of them holding together so as to make rails of any sort. The reason of the difference, I am informed, was that the first came from a vessel lined with Stourbridge brick, and the second from the same vessel with an improved lining of gannister, which, though much more enduring, no doubt had the effect of preventing any elimination of phosphorus. It is unfortunate that no analysis of the second lot of steel exists. For railway tires steel has superseded iron almost as completely as for rails, and there is a movement in favor of steel ships, which in all probability will extend; and when, as I suppose will be the case some day, the change is encouraged, rather than retarded, by the Board of Trade and other controlling bodies, iron plates will probably be to a great extent, if not altogether, supplanted. For boilers, steel—Bessemer and Siemens-Martin—is in extensive use. Mr. Adamson, whose very able paper at the Paris meeting will not soon be forgotten, and who, I am glad to say, is about to add to it for our advantage, was one of the first, if not the first, to introduce steel boilers on a large scale, and we have it upon his high authority that the result is most satisfactory. In the case of boilers no one can wish for anything but the greatest strictness to insure high quality, and therefore safety from accident; but it is difficult to see the advantage of insisting, as is the case at present, upon a like high and costly quality for the hulls of ships. The iron supplied for this purpose is certainly not of boiler-plate quality, and I believe it is admittedly good enough. Steel of fair quality is stronger than iron even after allowing for the reduced thickness permitted, and it is more reliable otherwise. There seems then no good reason why anything more should be demanded.

IS REMELTING NECESSARY?

Quite as unnecessary as the difficulties put in the way of steel for merchant ships is the objection to the taking of fluid cast iron direct from the blast furnace to the Bessemer converter. An opinion obtains in some quarters that the direct process is less under control, and the resulting steel may, therefore, be less uniform than by the old mode of mixing several brands of iron and remelting in the cupola. Closer inquiry will show that this opinion is erroneous, and that remelting is less reliable than working direct. In the large way the only guide the practical man has is eye-judgment. Samples can be, and in all properly managed establishments are, frequently analyzed; but I need not say that it is impossible to examine chemically more than an infinitesimal proportion of the pig iron used, the bulk of which can have nothing in the way of examination beyond the careful inspection by sight of an experienced pig sorter. In old times, when nothing more than this inspection was obtainable, it was a good additional safeguard to mix together half a dozen reputable brands of pig iron, that were very unlikely to be all wrong in the same chemical direction, and probably differed, when they differed at all, in ways that corrected each other; but the accurate knowledge of skilled furnace managers, advised by chemists whose everyday business it is to specify the proper mixtures for the blast furnace and keep out unsuitable materials, is far more certain. Given the proper ingredients, the blast furnace cannot make bad iron, unless its mechanical working be disturbed by neglect or accident, which those accustomed to furnaces would find out quickly. Indeed, before the evil can have gone far the blast furnace itself and the converter may be depended upon to make prompt protest and compel the necessary attention. My conviction is that, setting aside all question of cost, and considering only uniformity of quality, there is danger in remelting, and security without any disadvantage otherwise in the direct process. Those who, like myself, profess only to be what, for want of a better name, we call practical ironmakers, are already greatly beholden to the theorists, to whom we must continue to go for accurate information, and whose directions we must carry out as best we can. In due time there will no doubt arise a race of composite men, who, having both theoretical and practical knowledge, will be abler and more efficient than we, the one-sided men of the present.

THE NEW STEEL MAKING PROCESS.

I cannot forbear to mention specially the important improvement, patented and already brought to some practical success by Messrs. Thomas and Gilchrist, for the dephosphorization of ordinary pig iron, thereby fitting it for use in the manufacture of steel. (Applause.) The essence of the improvement is a substitution of a basic lining for the ordinary gannister lining of the Bessemer converter. The idea is not now brought forward for the first time. On the contrary, the principle has long been known and availed of in the old process of puddling. In 1872 Mr. Snelus patented the use of basic linings for furnaces and Bessemer converters, and I have no doubt it is in the recollection of many of you that more than one eminent metallurgist at home and abroad also recommended the change. Those who tried it, however, one and all, encountered difficulties of a practical kind which stopped them. These difficulties, it is believed, have now been overcome. Early last year, at the works of the Blaenavon Iron Company, Monmouthshire, experiments were made by Messrs. Thomas and Gilchrist, with the aid of Mr. Martin, the linings being silicate of soda and limestone, which, however, proved unsuccessful because unenduring. A larger trial at Dowlais shortly afterwards had the same result, though in all cases the phosphorus was eliminated, the difficulty being to maintain the lining, which was rammed into its place in the ordinary way. To meet this difficulty, bricks of ground magnesian limestone, burnt to great hardness, were made at Blaenavon, and proved by actual experiment to be efficient; but unexpected circumstances connected with the company's affairs stopped the proceedings. Messrs. Bolckow, Vaughan & Co., of Middlesborough, with their usual enterprise, undertook further experiments in a large way, and seem to have succeeded. Knowing as you all do that only a small percentage of the pig iron in the world is suitable for making steel by means either of the Bessemer or the Siemens-Martin process, I will not weary you by enlarging upon the enormous advantage of an improvement that promises to make available almost all kinds of pig iron. At some recent experiments, to which Messrs. Bolckow, Vaughan & Co. were good enough to invite the leading members of the local iron trade, the blowing was in every way successful, and the quality of steel produced excellent. The samples examined, several of which were made in our presence, varied in temper from the mildest and softest to the ordinary hardness for rails, any particular sort being produced at will. Samples to test were very easily taken, and it seemed that the exact temper of metal desired could be produced with certainty. The converter, lined with radial bricks of magnesian limestone, is said to stand well, and the necessary addition of cold basic material to the charge and during the blow, though, of course, it lowered the temperature at first, did not make a difficulty. As yet there cannot have been any accurate ascertainment of waste, cost of lining, etc., but these are unlikely to be seriously against the new method. One of the large vessels at Eston, lined with basic bricks, has been set to work, and it is believed that before long the cheap pig iron of Cleveland, without admixture other than that of spiegel, which is common to all, will be extensively used for making steel. The North-Eastern Railway Company has already ordered a considerable quantity of rails made from Cleveland steel, which will be manufactured under the supervision of their eminent engineer, Mr. Harrison, whose report will be as eagerly looked for as it will be relied on implicitly. If what is expected to be in the main realized, great changes are likely. The manufacture of steel rails, which at present is much spread over the land, will, in all probability, grow in the localities where the necessary raw materials—iron ore mainly—are abundant and cheap, with the sea not far away. Our former president, Mr. Bell, whose name will always be associated with blast furnaces, the theory and practice of which he has so thoroughly mastered and so lucidly explained, in communications not only to our institute, but also to other scientific bodies, has for a long time been working to adapt Cleveland iron for steel-making, with, it is believed, considerable success, and I have no doubt it will be very agreeable to you if he can, during the coming discussions, describe his system and the results he has achieved.

STEEL-MAKING ECONOMIES.

Before Mr. Bessemer's invention, almost everything depended upon the cheapness of fuel and labor, the differing costs of ironstone, though important, being less vital. To make one ton of finished iron, taking rails and other kinds together, required on the average about six tons of coal and the manual labor of many skilled men. For steel much of this is unnecessary; the consumption of coal per ton of finished steel is already less than three tons, and likely to diminish. When it is borne in mind that under the old system about one-third of all the coal raised in the realm was used at iron and steel works, and that the present total get is close upon 140,000,000 tons per annum, it will be seen how large a national gain this economy of fuel is. In the item of labor the improvement is still more striking; more than half the money cost of it is saved already, while the very hard work is in a great measure dispensed with, as is also the need of long special training, without which the old iron-making processes could not be carried on.

THE FUTURE OF IRON.

I need not say that there is yet a large amount of hand-puddling, which probably will continue for a long time. It will, I believe, steadily abate in amount, and I cannot doubt, though I say it with reluctance, knowing that the opinion is a little unpalatable to some excellent members of our Institute, that the day is at hand when, either by the Bessemer converter, or by the open hearth, or by some other steel-making apparatus, there will be produced, with absolute certainty, with comparatively light labor, and I hope with fair profit to all concerned, every kind, variation, and quality of the metal iron which we now rudely designate steel and wrought iron.

IRONMAKING FURNACES.

As is well known, Cort's furnace had a sand bottom, and the quality of the iron from it was never certain—inferior always to the iron of the old process, viz., that of working in a bath of fluid oxide, which, though very wasteful, produced iron of excellent quality, and therefore lived on in great part despite the new invention and its economies. For the Cort process excellent pig iron was made in cold blast furnaces from the best ores of the kingdom. The pig was refined in the old-fashioned refinery fires at great cost, and afterwards puddled on the sand bottom. The resulting wrought iron rolled badly, owing probably to the non-elimination of phosphorus, and could only be made into merchant bars of fair quality by repeated working in the mills, that involved heavy cost. A man of great ability, long since passed away—Samuel Baldwyn Rogers—proposed a cast-iron bottom for the puddling furnace instead of sand; and the suggestion brought not only the usual unbelief, but ridicule as well. That cast iron could be melted upon a cast iron bottom and the bottom itself remain sound, was held to be unreasonable; and the suggestions of Mr. Rogers was treated as one of the erratic notions of a man who, though known to have ability, was certainly very eccentric. After a time the first puddling furnace with an iron bottom was erected at Merthyr Tydvil, more than half a century ago, and so succeeded that it was adopted promptly. It was soon ascertained that puddled bars, instead of being, as formerly, unworkable stuff that would hardly hold together, rolled quite easily, and that a great deal of the costly mill working might be dispensed with. Gradually cheaper iron ores came into use, and there was less refining. Before many years, in most districts of the country, refineries were dispensed with, and pig iron puddled direct. The enormous heaps of discarded refinery and forge cinders, very good for many purposes as well as rich in iron, were worked up; and generally out of the invention of old Rogers came the greatest advantage to the manufacture of iron between the day of Cort and Bessemer. Mr. Danks hit upon a mode of lining with iron ore, which he preserved from destruction by charging oxide of iron with the heat; subsequently there was a further improvement in the same direction by the use of fluid tap-fettling run into a water-cased vessel, upon which it formed a lining. In both cases the improvement to the puddled iron was very great, owing to the removal of phosphorus; and but for the question of cost, and perhaps more than even that, the advent of ingot iron, the revolving puddling vessel would have done good service. It is not to the credit of practical ironmakers that they so long failed to see, and indeed did not see until an amateur pressed it upon them, that the non-elimination of phosphorus by the Bessemer and Siemens-Martin processes was for the same reason that Cort's furnace and the early revolving furnaces did not remove it, namely, the sand linings and bottoms.

FOREIGN AND AMERICAN COMPETITION.

It is often said—sometimes upon seemingly good authority—that in the manufacture of iron and steel this country is losing ground, and that there is danger of our being excelled by our energetic brethren across the Atlantic, as well as by our more scientifically educated neighbors on the continent of Europe. While I do not doubt that to maintain our leading position, and to acquit ourselves properly in the exact manufacture that has happily taken the place of the old finger-and-thumb business, we must, by every means in our power, add to, and make general, our knowledge of the composition and character of the materials we deal with, I do not think there is much ground for alarm. That England, which has so far originated and worked out almost all the great improvements in the manufacture, should hope to monopolize, or anything like monopolize, the iron and steel trade of the world is an unreasonable expectation, as I think it is also that in our own country, or indeed in other countries where there is commercial freedom, we are likely to be outstripped. (Applause.) Steel making has, as I have pointed out, rendered unnecessary much of the very hard labor for which our iron making population is so well fitted, and has brought into requisition nicety of manipulation, and an amount of general education that unfortunately is uncommon amongst us, as I am inclined to think it also is with the great mass of the people of other countries. In illustration of this is the fact that at present chemical terms and the appreciation of minute percentages, which even ten years ago were almost unknown, have become common and familiar; and I see no reason to fear that our rank and file of the future will be inferior to their competitors abroad. In the matter of machinery for making and shaping iron and steel, as well as the mechanical aids that become more and more essential, we have not yet been outstripped, and are not likely to be. After our late experience in France, where we were so openly and generously dealt with, we well know, if we did not know before, that there are splendid works abroad, and under the protection of import duties that keep away our competition, there are being established extensive and well-arranged concerns for the manufacture of steel; but while we must greatly admire the works of Creusôt, Essen, Terrenoire, Hayange, and many others, I do not con-

sider that in the department of steel making I understand, namely, that to meet the wholesome purposes of peaceful life, we are in even a second place. The making of steel for war purposes is a specialty I will not venture upon, my opinion being of little value.

WASTEFUL INGOT MAKING.

In the manufacture of ingots there is much loss and needless cost by reason of the rough system of teeming. The only guide the workman has is a chalk mark which can scarcely be worked to, and the ever-varying internal capacity of the moulds makes anything like accuracy impossible. The effect of this is that ingots are always cast too heavy, and crop ends are made instead of finished steel. There cannot be any insuperable difficulty in the way of showing by an indicator the exact quantity of steel teemed, and so enabling a skilled workman to run ingots of any desired weight. If this can be done, not only will there be considerable economy, but some of the unemployed iron-making mills will be available to roll steel in the old pull-over fashion, which has some advantages peculiarly its own. For heating, the regenerative gas-furnace is a great improvement; but it is believed, and I think, Mr. Bell holds the opinion, that the waste gases from the blast furnace are capable of developing more heat by their proper combustion than is necessary for all the steam-getting and heating after fluid pig iron, and that except the coke put in at the tunnel head of the blast furnace, there should be no expenditure of fuel. It is, I suppose, the fact that the ingot as it comes from the pit has more heat than is necessary for the rolling processes, the chilled outside being a shell holding a yoke of more or less liquid steel. Putting such ingots into an ordinary balling furnace is not therefore to heat them more, but to equalize the heat of the several parts. It would seem that for such equalization in a proper chamber only a very small expenditure of fuel, if any, is necessary; and if I am right, we may, in this part of the manufacture, expect a further important economy of coal.

BETTER ROLLING MACHINERY.

There is, I think, great need of improved rolling machinery We adhere too much to old types and systems, that were suitable enough for short lengths and light weights of a soft material, but which do not efficiently deal with hard ingots that are heavy and unhandy. Mere strengthening of engines and roll trains of the antique sort will not, I am convinced, do, even with the addition of the reversing engine, which is beyond question a great advantage for rolling long bars, though less suitable for short things like blooms and plates. Either by roll before roll, or by some entirely new system of shaping the steel, there will probably be before long a change for the better.

SCIENTIFIC TRAINING NEEDED.

In the scientific training of our overlookers and higher agents and managers, there is room for improvement, and it seems to me much to be desired that educated intellectual young men, who now hang listlessly about the professions and so-called genteel occupations, which promise to them at best only respectable poverty with the more than doubtful advantage of leisure, should break through the absurd old prejudice against seemingly rough work, and come over to the healthy business of iron and steel making, in which they may render the world good service, and obtain substantial remuneration by active employment of a very agreeable sort. Of the higher management of great works I scarcely dare to speak; but, as a rule, it really is not as it should be, and I hope to be forgiven for saying that there is need of amendment.

UNQUALIFIED MANAGERS.

For some unaccountable reason it seems to have become the opinion that gentlemen without special training, often engaged in other business of an entirely different sort, and coming only occasionally to the works as directors, can adequately and efficiently control and manage great manufactories. I am convinced that this is entirely fallacious, and that already there has been much hurt and loss by the system which in great part has obtained since manufacturing establishments became of unwieldy size, and passed from the hands of those who had grown up with them and understood every detail, into those of corporations and companies, unskilled in the necessary technical matters, without knowledge of the local circumstances, and wanting also the good personal relations that existed between their predecessors and the persons they employed. It is not easy to name a business much more complicated and fuller of detail than the real management of a great iron and steel works, which, to be efficient, should have the constant attention of a competent staff, led and actually controlled by a technically-trained manager, skilled in the special business, and possessing, if possible, the somewhat rare quality which Motley, the American historian, well describes as "a gift which no training and no culture can bestow, and which comes from above alone by birthright divine—that which men willingly call master—authority." It would be invidious, though not difficult, to show that where this kind of management has been the rule there is usually prosperity, and how many good and promising establishments have languished and are languishing for want of it.

FUTURE PROSPECTS.

There is, I think, a great future for iron and steel, not only in the few branches of the manufacture I have referred to, but in each and all of them, notably, I believe, in the employment of blown metal for castings, that with no great additional cost cannot fail to be greatly superior in strength to any made from untreated pig iron, however good; and it will be strange if, in the state of general education that is coming, men like Cort and Rogers, Neilson and Vaughan, Bessemer and Siemens, Bell and Menelaus, and other able men at home and abroad, who have made the art of iron and steel making what it is, do not arise in greater number to lead, improve, and strengthen it. That the workmen of our free country will be unequal to their duty, or fail in it, I do not believe; on the contrary, I hope and expect that, with better education and the great improvements it must bring, they will hold in the future, as they have held in the past, the foremost place for skill, endurance, and honest industry.

The president resumed his seat amidst prolonged applause.

Mr. Geo. J. Snelus read his paper on

THE REMOVAL OF PHOSPHORUS AND SULPHUR DURING THE BESSEMER AND SIEMENS-MARTIN PROCESSES OF STEEL MANUFACTURE.

In speaking of the elimination of phosphorus during the puddling process, Dr. Percy, in his well-known work on iron and steel, suggests that probably the phosphorus is eliminated by a process of liquation of fluid phosphide of iron from the pasty puddle ball. It may be worth while to quote the exact words, which are as follows: "Hence it is clear that by the direct action of the oxygen of the air upon molten pig iron, or even molten malleable iron, phosphorus is not eliminated in a sensible degree. To what, then, are we to ascribe the opposite result which occurs in the process of puddling in which there is powerful, oxidizing agency at work? Is it due to the effect of the liquid cinder? Although I cannot answer these questions in a satisfactory manner, yet I will enter upon a conjecture which I have long regarded as at least plausible. In the puddling furnace, the iron, as it 'comes to nature,' separates in a semi-solid or somewhat pasty state, and is not molten, as it is in Bessemer's process, the temperature in the former case being much below that in the latter, in which intense heat is developed by the actual combustion of iron. Owing to the formation and persistency of these pasty lumps in the puddling furnace, which, when collected into balls, extend considerably above the surface of the bath of cinder, opportunity is afforded for the liquation or sweating out of any fusible compounds of iron such as phosphide; and it is in this manner that I conceive the removal of the phosphorus may in a great measure be effected." Other writers upon the subject have adduced various causes to account for the remarkable fact that has been so long accepted as an axiom, that phosphorus could be almost perfectly removed by the puddling process, and not at all in the Bessemer converter; but I am not aware of any writer who, up to the time I made my statement at our London meeting last year, had publicly made known that the elimination of phosphorus in the pneumatic process depended entirely upon the basic nature of the slag. I was led to doubt the correctness of Dr. Percy's hypothesis during my studies of the puddling and refinery processes at Dowlais, and when I came to study the reactions which occurred in the Danks process, the particulars of which were laid before this Institute, I was struck by the fact that a good deal of the phosphorus was eliminated while the iron was in a fluid state. I communicated this observation to Dr. Percy in March, 1872, and at the same time I told him that I believed I had discovered why this occurred. On comparing analyses of iron and slags at different stages during the processes of Welsh refining, puddling, and Bessemer converting, I had previously noticed that while the phosphorus was comparatively easily removed in puddling, variable but generally small amounts were removed in refining, and none at all in converting, even if the iron were considerably overblown. A comparison of the slags from the three processes showed that while that from the puddling process was highly basic, that from the refinery was less basic, and that from the converter highly silicious. I also noticed that if a silicious fettling were used in the puddling furnace, or a very silicious pig iron used, the resulting metal was inferior; and hence while at Dowlais I strove to get a non-silicious pig for puddling and a non-silicious ore for fettling, and strongly recommended for this purpose the Campanil ore from Somorostro. For similar reasons, when consulted some years ago by Mr. Jenkins, of Consett, about the difficulty then experienced in keeping his iron for plates free from red shortness, I advised the use of a manganiferous ore moderately free from silica, which has, I believe, been used with success ever since. It is a well-known fact that silica is a much stronger acid than phosphoric acid, and that it will displace the latter from combination at high temperatures, and consequently, unless the silica is thoroughly saturated, there will be no base for the phosphoric acid to combine with, and, therefore, in contact with metallic iron at high temperatures, it will be reduced as fast as made, if, indeed, the phosphorus is oxidized at all under these conditions. These considerations led me to believe that phosphorus was removed in all processes just in proportion to the basic nature of the slag. Having these facts before me, early in 1872 I considered how I could obtain invariably a highly basic slag in the Bessemer converter, and I saw clearly that to obtain this result it was absolutely necessary to line the vessel with a basic or neutral material, and that no success could be expected so long as it was lined with gannister or any material containing a large proportion of silica.

Carbon occurred to me as an infusible neutral substance, suitable in so far as it would not affect injuriously any lime or other oxide that might be added to form a basic slag, but it had other serious drawbacks. Oxide of iron alone was too fusible to stand the intense temperature of the blow. While thinking over these things, I was fortunate enough to make the observation that certain kinds of lime, when submitted to very intense heat, became indurated and converted into a form in which they were no longer acted upon by water. The observation was made while performing the experiment recorded on page 249, vol. 2, of the Institute Journal for 1872. I had used a lime crucible for the experiment, which was to prove the power of free silicon to reduce oxide of iron. The crucible, embedded in lime, was submitted to the most intense heat of the Siemens steel-melting furnace at Dowlais. After the experiment I noticed that the lime crucible had turned brown, and was no longer alkaline to the taste, while at the same time it was quite hard and stony in character, and absolutely unaffected by water. It at once occurred to me that lime in some form or other was the material I was in search of for lining the converter. The well-known fact that it does not do to burn lime at too high a temperature if it is to be caustic also occurred to me. This is known to be particularly the case with magnesian limestone. Hence I specified magnesian limestone particularly.

From some experiments I made shortly afterward, I found that it was possible to make bricks out of lime or limestone, provided that when lime was used it was crushed quickly, compressed, and fired before it had time to absorb moisture, but it required intense temperature to consolidate them, particularly if the lime was very pure, while a small quantity of oxide of iron or other fusible base facilitated this. If lime was used, it was necessary to be well burnt first, otherwise the material was not porous enough to allow the small quantity of carbonic acid to escape without breaking up the brick. Finding, however, that crushed limestone lent itself with considerable facility to such a purpose as lining a converter, being plastic and binding when moist—like clay, in fact—it occurred to me to use it in this form, and to fire the vessel at a very high temperature. This was comparatively easy to do in a small converter, but I found considerable difficulty when I came to try a 7-ton converter. There did not appear to be any difficulty in forming a fusible slag in a vessel lined with lime, since this base combines with an excess of oxide of iron to form a fusible ferrate of lime. It was only necessary, if the waste of iron during the blow were insufficient to provide the necessary oxide of iron to form a fusible slag free from silica, to add oxide of iron specially, to which course there could be no objection, while the presence of the oxide of iron would doubtless be advantageous in other ways. At the same time, lime being a basic body, would not be so readily acted upon by the oxide of iron as a silicious body would be. Following up these conclusions, I made several blows in a small (2 cwt.) movable converter soon after I went to the West Cumberland Works.

The results of these experiments I have shown to many private friends, and I have now the pleasure to lay the details before the Iron and Steel Institute. The results with the small vessel were very satisfactory, as will be seen by reference to the analyses, and there can be no doubt as to the perfect elimination of phosphorus and sulphur when the slag is kept thoroughly basic. In order that there might be no doubt on the subject, I lined the same small vessel with gannister in the usual way, when I found that the phosphorus refused to leave the metal, just as in the ordinary converter. I therefore patented the use of lime and limestone, magnesian, and otherwise, in all the various forms in which it occurred to me that it was possible to use it, for "the lining of all furnaces in which metals or oxides are melted or operated upon while fluid;" and I especially called attention to its use in the Bessemer converter, giving some details of the results of its use under these circumstances.

I was perfectly well aware that limestone had been used years ago for lining the sides of puddling furnaces, but its use for this purpose was a very different thing, because the metal in the puddling furnace does not remain fluid at the end of the operation, for which reason it did not answer satisfactorily, and, as is well known, its use for this purpose is almost entirely discarded.

I felt, however, that although I had found the solution of the problem so long and anxiously looked for, my plans required to be tried on a larger scale before they were fully laid before the public, not because there could be any question that the same chemical action that succeeded with 1 cwt. or 2 cwt. of metal was likely to be reversed when operating upon tons, but because I felt that there were many points of detail that would require to be worked out. These details have been filled up by subsequent workers, but it is gratifying to me that the complete success now attained follows so closely some of the lines then laid down. I shall doubtless be asked why I did not at once follow up my discovery, and put my plans into practice. My answer is that I had just taken the management of a concern, the interests of which were opposed to the solution of this problem, and therefore, having secured the ground by patent, I was compelled to wait a more favorable opportunity for putting my plans into practice. There was also a good deal of skepticism amongst those interested as to the possibility of solving the problem, and none seemed to care to take it up. The details of my experience are as follows:

In the first blow the converter was lined with ground limestone in a plastic state, and fired at as high a temperature as possible for a considerable time. A small cupola was also lined in the same way. About 2 cwt. of Middlesborough pig iron, in which the phosphorus was about 1½ per cent., was melted in the cupola, then run into the converter, and blown in the usual way. The metal blew well, and some spiegel was added at the end of the blow, which accounts for the rather high carbon in the steel. On analysis the resulting steel was found to contain:

Iron, 97·842 per cent.	While the slag contained—
Carbon, ·8.	
Silicon, trace.	Silica (over), 6·0.
Sulphur, trace.	Lime, not estimated.
Phosphorus, ·018.	Phosphoric acid, 3·2 per cent.

In order to try whether sulphur could be removed at the same time, a second blow was made in the same apparatus, a quantity of melted mottled hematite being run into the converter, and lumps of gray Cleveland pig were thrown in with it to afford additional silicon. In order to trace the reactions which took place, samples of metal and slag were taken from the converter during the blow and submitted to careful analysis. The metal was scarcely full blown, as will be seen from the final sample. Those taken during the blow yielded the following results:

	1st Sample of Metal.	2d Sample of Metal.	3d Sample of Metal.
Iron	—	98·29	—
Carbon	3·0	1·25	·06
Silicon	—	·11	·042
Sulphur	·346	·274	·169
Phosphorus	·864	·314	·231
CORRESPONDING SLAGS.			
Fe_2O_3	12·1	—	7·1
FeO	23·8	41·9	39·2
Silica	44·8	25·0	19·2
Lime	14·8	—	24·0
Phosphoric acid	2·3	7·25	7·0
Sulphur	·3	·352	·512

It will be noticed that the silica in the slag is rather high in this case, and that the imperfectly blown sample, No. 3, has not quite lost all its phosphorus and sulphur. The gradual diminution of these elements, however, is very conspicuous, and no doubt the fully blown sample would have been sufficiently pure for ordinary purposes. The third blow was made from a mixture that was tolerably free from phosphorus and sulphur to begin with, in order to see whether it was possible to remove the last trace of these elements. This was a very perfect blow, lasting just 15 minutes and the exact times at which the samples were taken were noted. Further, in order to test the steel, some of the fully blown metal was melted in a crucible with 6 per cent. spiegel, and tested in various ways.

3d Blow.—About 100 lbs. No. 2 Bessemer pig and 30 lbs. gray Cleveland were melted together, and then blown in the same vessel.

The results were:

Metals.

	1st Sample, taken 2 minutes from start.	2d Sample, taken 5 to 6 minutes from start.	3d Sample, taken 10 to 12 minutes from start.	4th Sample, at end of blow, taken 15 minutes from start.	After melting, with 6 per cent. spiegel in crucible.
Iron	—	—	—	—	98·44
Carbon	—	—	—	trace	1·30
Silicon	1·505	·046	0·84 part slag	·019	trace
Sulphur	·109	—	—	·030	·032*
Phosphorus	3·27	·094	·076	·003	·052*

* The increase in these elements was due to the spiegel added.

Corresponding Slags.

Fe_2O_3	—	—	53·8	11·42	—
FeO	—	—	—	49·00	—
Silica	—	—	10·2	10·50	—
Lime..... ...	—	—	33·0	25·00	—
Phosphor. acid	—	—	2·88	2·05	—
Sulphur	—	—	—	·26	—

The full-blown metal was fluid, and set moderately sound. It forged and rolled well, and could be easily welded. It stood hot and cold tests well. A larger vessel was then prepared, and about one ton of highly manganiferous pig blown in it, containing about 3 per cent. phosphorus.

The blown metal contained—		And the slag contained—	
Carbon	·4	Iron	33·7
Phosphorus	·006	Silica	12·65
		Lime.	28·2
		Phosphorus	·519

The metal at the end of this blow was perfectly fluid, and the lining of the vessel remarkably good. The arrangements for tilting this vessel were imperfect, and the metal had to be allowed to set in the vessel. Lastly, a charge of four tons of metal was blown in a large 7-ton converter, but the lining had not been sufficiently heated, and the metal blew so cold that it could not be poured. As the vessel could not be spared for another blow, the experiments were suspended at this point. It appeared that the only serious difficulty was in getting the lining of the vessel, when put in in the ordinary way, thoroughly burnt, and it occurred to me that a way out of the difficulty might be found in making bricks out of the material and burning them beforehand, but I did not go beyond satisfying myself that this was a perfectly feasible plan, and, having to devote all my energies at this moment to reorganizing the West Cumberland Works, further tests were put off. Although no experiments were made in a Siemens furnace, all chemists will agree that the reactions occurring in the Bessemer converter could be more easily obtained in the open-hearth furnace. Some time afterward Dr. Siemens was good enough to have his experimental rotator at Birmingham lined with limestone in a similar way, but it did not answer, for two reasons, in my opinion—first, the limestone was a peculiarly rich one, and would not bind well before burning; second, the temperature that could be obtained in the rotator was not sufficiently high. It is essential that the material used for lining the vessel, and for bottoms, should be as free from silica as possible, as some of this must go into the slag, and it appears to be necessary to have a large amount of base to saturate the silicic and phosphoric acids present in the lining material or bases added to form slag, or formed from the oxidation of the silicon and phosphorus in the iron. Unless this is done the slag becomes too acid to allow the phosphorus to be eliminated. This is seen to some extent in Blow No. 2. It is a question how much base is required, but I am inclined to believe that there must be sufficient to form the basic silicate and tribasic phosphate. Mr. Roe, who is at present engineer to the Consett Works, was engineer at West Cumberland at the time these experiments were made, and assisted in performing them. In conclusion, I have the pleasure to place before the meeting what I believe to be the first sample of Bessemer steel made entirely from Cleveland iron by one operation, in which the phosphorus has been reduced to a mere trace. A portion has been forged into a chisel, while a rough specimen of the same has still part of the lime lining attached to it. With the samples I have placed the original wrapper bearing the date when the sample was made, and also my note book with the original entries of the details of the analysis.

DISCUSSION.

Dr. Siemens, in opening the discussion, said they had to distinguish between two things, namely, the proposition of the basic lining as a proposition, and the results which they were told had been obtained with such a line. In 1863 he, in conjunction with a friend, M. Caron, made several experiments for the purpose of producing steel, and in discussing with his friend the question of lining, his friend strongly maintained that, in order to carry out the process properly, they ought to adopt a basic lining, and he suggested a material—bioxide—which he (Dr. Siemens) still believed to be the best material for that purpose. The method pursued was this: The bioxide was put into the furnace, but they did not get the lining to stand when the fluid charge came upon it. He then made bricks of bioxide, of which he had at present a large quantity at his works at Birmingham, and with them he succeeded in obtaining a lining that would stand after a series of charges, and the results had been good, although he had to confess that he had no such marvelous results to put before the conference as were stated by Mr. Gilchrist in his paper. Before abandoning the idea of basic lining, he also tried lime, which he mixed with clay and other binding substances, but he found the difficulty increased very much in using that. He also used a line made of magnesia, formed into bricks burned at a very high temperature, and that produced a magnificent line, but it was an expensive line, and on that account was not commendable. But the reason why he did not persevere with the basic line was this, that although for a few charges it seemed to stand exceedingly well, yet the iron appeared to corrode into it loosely, and the lining became disintegrated and could not be repaired. In dealing with the lining they had only to put in a few shovelfuls of pure sand where the lining was defective, ten minutes of intense heat was applied, and they had again a perfect line, while in the corrosive lime the line had to be taken out entirely and changed. He had used the bioxide line in a radiating furnace, and in a paper he read some years ago he pointed out the advantages of the bioxide line. In looking upon the proposition before them that day he found that there was something missing in the papers which had been communicated to them. They knew that basic material would absorb phosphorus, and the president in his address had already called attention to the cycle through which they seemed to wander in going from an imperfect to a perfect line. It is probable that in making steel they were only passing from the first stage to the second of a good line. But the difficulty was more serious. In the puddling furnace the temperature did not exceed the melting temperature of the bioxide itself, and it therefore could be easily repaired, and it lasted a considerable time, but he apprehended that in all basic lines in the converters a good deal of difficulty and expenditure would be incurred in the renewing of lining. He repeated the opinion he had already expressed, that it was not the amount of basic material necessary to absorb from a charge of Cleveland pig the amount of phosphorus which it contains such a serious item in the expenditure as to make it still an open question whether they ought not better go on using West country iron for steel—[applause]—they must bear in mind the materials of which those lines were composed. The question really was whether limestone lining would carry any amount of phosphorus away from the iron which was to be taken out, and they had to ask themselves how much bioxide was necessary to accomplish the effect contemplated, and how much of the bioxide was produced in overflowing the charge. [Applause.]

Mr. Richard proposed giving a very brief history of the experiments which he had made in order to produce a basic brick which would operate satisfactorily. What was wanted was to make a basic brick; but this was much easier said than done. First of all a large number of limestone quarries were visited before one could be found adapted for the purpose. Limestone containing 5 to 6 per cent. of silica, 2 per cent. of aluminum, 35 per cent. of carbonate of magnesia, and 54 per cent. of carbonate of lime, was found to be a very proper material. It was only necessary to grind it up, moisten it with water—similar to clay—and then mould the bricks by hand in the ordinary way. It was found in burning that the bricks increased in size, and it was found in the end necessary to give the kiln a down-drop, so that the top bricks should be burnt first. Bricks which had not been sufficiently burned when exposed to the air went to pieces. The bricks—a sample of which was shown—were burned in a kiln lined with gannister bricks, though it was only fair to say that some of the bricks were not so good as the sample. The limestone did not cost more than 7s. per ton, and the cost of the brick did not exceed the cost of the gannister bricks. [Mr. Richard, by means of a diagram, showed the results of his experiments in eliminating phosphorus from Cleveland iron, the result of which was that the whole of the phosphorus was eliminated.]

MINERS' LINGO.

DEFINITIONS OF THE TERMS USED BY THE MINERS.

BELOW will be found a collection of miners' terms and their definitions, as given by Mr. M. B. Carpenter in his new mining code:

Adit—A level, a horizontal drift or passage from the surface into a mine.

Alluvium—A deposit of loose gravel between the superficial covering of vegetable mould and subjacent rock.

Apex—The top or highest point of mineral.

Argentiferous—Containing silver.

Ascension—The theory that the matter filling fissures was introduced from below.

Assay—To test ores by chemical or blow pipe examination.

Auriferous—Containing gold.

Bed—A horizontal seam or deposit of mineral.

Blende—An ore of zinc, consisting of zinc and sulphur.

Bonanza—Fair weather; a mine is said to be *en bonanza* when it is yielding a profit.

Bowlders—Loose, rounded masses of stone.

Breast—The face of a tunnel or drift.

Cap—A vein is in the "cap" when it is much contracted.

Carbonates—Soft carbonates; salts containing carbonic acid, with a base of lead. Hard carbonates, the same with iron for a base.

Cheek—The side or wall of a vein.

Chimneys—The richer spots in lodes as distinguished from poorer ones.

Cinnabar—Sulphuret of mercury.

Claim—The space of ground located and worked under the laws.

Chlorides—A compound of chlorine and silver.

Contact—A touching, meeting, or junction of two substances, as rocks.

Contact Vein—A vein along the contact plane of, or between two dissimilar rock masses.

Country—The ground traversed by a vein.

Country Rock—The rock masses on each side of a vein.

Course of a vein—Along its length (see strike).

Crevice—A narrow opening, resulting from a split or crack; a fissure.

Cribbing—A timber or plank lining of a shaft; the confining of a wall rock.

Cropping Out—The rising of layers of rock to the surface.

Cross Cut—A level driven across the course of a vein.

Cut—To intersect a vein; open cut, a level without a covering driven across the course of a vein.

Debris—Fragments detached from rock or mountain.

Descension—The theory that the material filling veins came in from above.

Diggings—Name applied to placers being worked.

Dip—The slope, pitch, or angle which a vein makes with the plane of the horizon.

Diluvium—A deposit of superficial sand, loam, pebbles, gravel, etc.

Ditch—An artificial water course, dug in the earth; a flume or canal.

Drift—A horizontal passage under ground.

Dump—A place for deposit of tailings or waste rock.

Dike—A wall like mass of mineral matter filling fissures.

Exploitation—The working of a mine; the amount of work done.

Face—The end of a drift or tunnel.

Fault—A displacement of strata of veins so that they are not continuous.

Feeder—A small vein joining a larger one.

Fissure Vein—A fissure or crack in the earth's crust filled with mineral matter.

Float—Loose rock or isolated masses of ore, or ore detached from the original formation.

Foot Wall—The layer of rock immediately under the vein.

Forfeiture—A failure to comply with the laws, prescribing the quantity of work.

Galena—Lead ore; sulphur and lead.

Gangue—The substance inclosing and accompanying the ore in a vein.

Gash Vein—A vein wide above and narrow below.

Geode—A cavity studded around with crystals or mineral matter; a rounded stone containing such a cavity.

Gold—A reddish yellow colored metal.

Hanging Wall—The layer of rock or wall over a lode.

Heading—The vein above the drift.

Headings—In placer mining, the mass or gravel above the head of the sluice.

Horse—A mass of rock matter occurring in or between the branches of a vein.

Hydraulicking—Washing down a placer claim by the use of hose or "giant nozzle."

Impregnation—Metallic deposits having undefined limits and form.

Incline Drift—An inclined passage under ground.

Infiltration—The theory that vein filling was introduced as mineral water.

Injection—The theory that vein filling was introduced by an igneous fluid, and solidified.

In Place—A vein or lode inclosed on both sides by fixed and immovable rock.

Lagging—The timber over and upon the sides of a drift.

Length—A certain portion of the vein when taken on a horizontal line.

Level—A horizontal passage or drift into a mine from a shaft.

Little Giant—A jointed iron nozzle used in placer mining.

Lode—Aggregations of mineral matter containing ores in fissures.

Matrix—The rock or earthy matter containing a mineral or metallic ore.

Metallurgy—The science of the reduction of ores.

Mine—An excavation in the earth from which mineral substances are dug.

Mill Run—A test of a quantity of ore after reduction.

Nodule—A rounded mass of irregular shape.

Ores—Compounds of metals with oxygen, sulphur, arsenic, etc.

Outcrop—That portion of a vein appearing at the surface.

Patch—A small placer claim.

Placer—A gravelly place where gold is found; includes all forms of mineral deposits excepting veins in place. Section 2,329, Revised Statutes United States.

Pocket—A rich spot in a vein or deposit.

Prospecting—Searching for new deposits, also preliminary explorations to test the value of lodes or placers.

Quicksilver—Mercury, used in sluices to catch gold.

Riffle Blocks—Wooden blocks set on end in a sluice, with interstices for catching gold.

Segregations—Aggregations of ore having irregular form and definite limits.

Selvage—Thin band of earthy matter between the vein and walls.

Silver—A soft, white metal.

Slickensides—Smooth, polished surfaces of walls caused by violent trituration.

Sluices—Boxes joined together, set with riffle blocks, through which is washed auriferous earth.

Smelting—Reducing the ores in furnaces to metals.

Stamps—Machines for crushing ores.

Stope—One of a series of steps into which the upper surface of an excavation is cut; to excavate in the form of steps above a drift.

Stoping—The act of stoping or breaking down the surface of an excavation with a pick.

Strata—A series of beds of rock.

Strike—The extension of a lode in a horizontal direction.

Stull—A framework covered with timber or planks, to support rubbish in working a stope.

Sublimation—The theory that the vein matter was introduced in a gaseous condition.

Sump—That part of the shaft below the platform used for receiving water.

Tailings—The refuse matter discharged from the end of a sluice.

Tunnel—A level, driven at right angles to the vein, which its object is to reach.

Vein—Aggregation of mineral matter in fissures of rock.

Walls—The sides next to the lode.

Whim—A machine for raising ores and refuse.

Winze—A shaft sunk from one level to another.

ORNAMENTING TEXTILE FABRICS.

By WILLIAM W. CARPENTER, St. Louis, Mo.

THE process of ornamenting textile fabric practiced by me consists in placing upon the textile fabric to be ornamented natural or other leaves, or other vegetable matter, in the positions desired, and spraying on to the textile fabric and leaves or other vegetable matter a coloring mixture composed of coloring matter and a volatile fluid. Upon the removal of the leaves or other vegetable matter their outlines or figures will be found on the textile fabric represented in the general color of which the textile fabric is composed, while surrounding these figures will be the colors that have been sprayed upon the textile fabric. The portions of the textile fabric covered by the leaves or other vegetable matter, being protected by them, are not colored by the sprayed mixture.

The spray coloring that I use in my process is composed of a volatile fluid, as naphtha, turpentine, gasoline, etc., with which is combined lampblack in proper proportions to make the mixture of any consistency desired. By the use of this mixture the ends of leaves will not turn up, as is the case when any other mixture known is used, but the leaves will remain throughout the process in the exact position in which they were originally placed.

The use of this mixture obviates the necessity of pinning the ends of the leaves to the article upon which their figure is taken, as must be done when any other known mixture is used. Simple and convenient methods of spraying the spray mixture on to the textile fabric are brushing the mixture through the meshes of a sieve with a brush, or by the use of an atomizer or of a syringe with a finely perforated bulb nozzle. Leaves of any desired figure or objects of any desired shape may be used, and the same may be placed in any desirable position on the textile fabric.

The grouping may be as simple or as elaborate as the taste dictates.

ON SILK DYEING.

By MARIUS MOYRET.

FROM the most remote antiquity down to our own days all nations, even the most savage, have a desire to color textile fibers and to embellish the tissues destined for their garments, etc., with varied designs.

Ancient history tells us even of certain very renowned colors, such as the "purple," which was the symbol of power and command. The Chinese, a stationary people of a very ancient civilization, show us still in our times the art of silk dyeing as it was carried out 2,000 years ago, and probably much earlier.

A well applied dye has not merely for its result the embellishment of tissues, but even their amelioration. In our large towns garments entirely white would soon become unfit for use.

To explain the various phenomena of dyeing, towards the end of the last century Hellot, Pileur, Apligny, and Macquer, and in our own days Persoz and Crum, have propounded various and contradictory theories. In fine, after studying these theories, it must be admitted that none of them suffices to explain everything, and that it is necessary to apply all of them in different cases.

Dyeing may be generally divided into two portions, direct and indirect, which will be studied and discussed in the subsequent paragraphs.

DIRECT DYEING.

Direct dyes (substantive dyes) are those where the coloring matters brought in contact with silk combine with it or become fixed without the aid of any intermediary agent.

Direct dyes may be subdivided into three classes:

1st. Those where the color is developed upon the fiber. Indigo, as applied in the vat, is the only type of this class.

2d. Those where the coloring matter is absorbed by the silk, but without true combination.

3d. Those where the coloring matters are fixed profoundly upon the silk, or rather combine with it so intimately that both reciprocally lose their characteristic properties.

In the first class, that of the blue vat, the fiber of the silk is saturated with an alkaline colorless solution of indigo, which, on aeration is converted, on the fiber itself, from soluble indigo-white to insoluble indigo-blue, the precipitation taking place in the silk.

In the second class the silk fiber behaves with coloring matters just as wood-charcoal and bone-black behave with certain gases and dissolved substances.

The coloring matters are then fixed by the porosity of the silk, but neither undergoes any modification. There is no true combination, and in several cases, as in that of extract of indigo, it is easy to strip the shade and to restore the silk to its original condition.

Thus, if a color like extract of indigo has been applied alone or slightly acidulated, it may be removed from the fiber by simple treatment with a lukewarm alkaline bath, just as might be done with wood-charcoal or bone-black.

The third case comprises substances which have, in the proper sense of the word, an affinity for silk, and to this class belong especially the astringent matters of tannins. These substances possess a great affinity for proteic matters, with which they combine in enormous proportions, whilst in the second case only small quantities are concerned.

Both the silk and the astringent principle lose their properties in part, and it is impossible to restore them completely.

INDIRECT DYEING.

Indirect dyes (adjective colors) are those which require an intermediate agent or mordant to aid in the fixation of the coloring matter upon the fiber. Such dyes are generally applied in two operations, the first consisting in fixing the mordant, and the second in depositing the color upon it.

Indirect dyes are subdivided into two classes:

1st. Indirect dyes properly so called.

2d. Dyes depending on the formation of an insoluble metallic precipitate upon the silk, in a greater or lesser quantity.

To the former class belong the bulk of the old dyes with woods, roots, etc. Silk, for instance, dyed directly in a solution of logwood, would take a mere dull fawn-colored stain; but if previously mordanted with iron, copper, or alumina it will take, according to the mordant, black, blue, or violet shades, true precipitates which are formed upon the fiber, and which agree in color with the precipitates (lakes) formed by mixing the same materials in test glasses. Such dyes are generally fast as against alkaline reagents which usually strip direct dyes. With acid reagents the reverse is the case. In general acids aid direct dyes, and reduce indirect dyes by laying hold of the metallic oxides which have been fixed.

In indirect dyeing with mordants the attention of the dyer must be turned towards depositing them perfectly level, otherwise when the dye comes to be applied the consequences will be disastrous.

The mode of action of the mordants, which are always metallic, will be seen in the following paragraph. There is, nevertheless, one case where the mordant is organic, formed of tannins, as occurs in dyeing black.

Indirect dyes, which depend on the formation of a metallic precipitate, play but a small part in silk dyeing. The precipitates formed upon silk by the fixation of a first salt, such as a salt of iron or of lead, followed up under suitable conditions by the action of a second salt, such as yellow prussiate, bichromate of potash, etc., in order to form blues or greens, affect its luster and its softness too much; so that these methods, employed for cotton, are abandoned or nearly so as regards silks.

Silks, therefore, to sum up, are dyed directly or indirectly.

Directly: either by fixation of a soluble color which becomes insoluble through oxidation or exposure to the air, as in the blue vat;

Or by a simple action of porosity akin to the decolorizing power of bone-black. Examples are the artificial colors—indigo-carmine, etc.—in general; in this case acids facilitate the fixation of the color, and alkalies tend to loosen or strip it. There are a few exceptions to this rule, such as orchil and annatto, which work on in an alkaline solution;

Or by chemical affinity, properly so called, to which class belong the astringents; and the properties both of these bodies and of the silk are modified by an act of true chemical combination.

If silks are dyed with certain coloring matters by the aid of an indipensable intermediate, the dyeing is indirect, and the intermediate takes the name of a mordant, and consists in a metallic oxide. The shades with one and the same coloring matter vary according to the oxide, and as a rule such colors support alkalies but not acids.

Indirect dyes may occur in several distinct cases.

Either they are produced by the action of the coloring matter upon an oxide fixed as a mordant, the most common case;

Or by the fixation of a metallic precipitant upon a metallic compound previously fixed;

Or by the fixation of a metallic precipitate upon a body of the astringent class, such as catechu or galls deposited upon the silk.

Mordants, which have been in use for a long time, still constitute one of the bases of dyeing, despite the invasion of the modern artificial colors. The latter, indeed, give all the colors of the rainbow without, like the wood colors, undergoing changes under the influence of metallic salts, and have almost expelled them from the department of clear, bright colors; but it is not the same with saddened colors and blacks, which are generally produced by the use of several of the wood colors modified by the action of the mordants.

The commonest mordants are those of alumina, tin, iron, and copper; then follow those of chrome, manganese, mercury, etc., less used in silk dyeing.

ALUMINA MORDANTS.

To fix alumina upon silk the only agent formerly known was alum.

At present, besides the different qualities of alum, the dyer has at his disposal other aluminous mordants, such as sulphate of alumina, hyposulphite of alumina, acetate of alumina, aluminate of soda, and, quite recently, a solution of alumina in methylamine has been proposed. If this last can be produced at a low price, it will be the most active aluminous mordant.

Alum is the salt most commonly used, and it occurs in three sorts:

Roman alum.

Alum in the Roman style.

Ammonia alum.

The two former are potash and alumina alums, of the formula $KO, SO_3 + Al_2O_3, 3\,SO_3 + 24\,HO$.

Roman alum, the kind most anciently known, is prepared from a mineral named alunite, and is now rare and much sought after. It has a rusty tint, formed by a deposit of (insoluble) peroxide of iron. This rusty tint forms, moreover, in consequence of the manner of manufacture, layers alternating with other layers perfectly white.

Later, when alum, identical with that of Rome, was made in France and elsewhere, in order to find a market for it, it was necessary to give it this rusty tint by means of some red ocher.

Roman alum is valuable on account of its great purity, especially its freedom from iron, which is a very pernicious ingredient; for if alumina tends to give bright colors, iron, even in very trifling proportions, tends to sadden. An alum intended for delicate colors ought to show no immediate blue coloration or precipitate if tested with prussiate of potash. As to the rusty tint which characterizes Roman alum, although due to iron, it is entirely without action upon the coloring matters. Being perfectly insoluble in the dye baths, it settles to the bottom like so much sand.

In our days ammonia alum, on account of its low price, is substituted for Roman alum. The first and commonest kind is somewhat irony, but may serve for blacks (?) and saddened shades. The second kind, refined alum, is obtained by recrystallization of the former. It is less ferruginous and higher in price.

Finally, the third kind, or so called double refined alum, is obtained by a further recrystallization of the former. It is pure enough for the most delicate shades and may rival Roman alum.

[We must here remark that ammonia alums are becoming scarcer, and potash alums are regaining something of their old supremacy.]

The process of aluming silks may precede all the dyeing, or it may take place in the course of the operations. In the former case the alum bath may be permanent, while in the latter it is generally run off after using.

To alum silks they receive three or four turns, and are then put to steep at the bottom of the alum beck, where they are left for several hours, ordinarily over night. The solution of alum should be saturated in the cold, and should be refreshed from time to time with a boiling and concentrated solution of alum. The same end may be gained by keeping in the beck, just beneath the surface of the liquid, a bag full of fragments of alum, taking care to stir up the bag well before the introduction of each lot of silk.

For aluming, the silk ought always to be suitably moistened, the skeins containing about their own weight of water. If the silks were introduced dry, they would dye badly, and in some cases they would not very easily become wetted by the dye bath. A consequence of this indispensable degree of humidity is, that permanent baths are gradually weakened and require an addition of coloring matter, etc., if they are to be maintained at one constant strength.

Alum is applied in the cold. When the silks are taken out, they must not be dried off from the alum, as they would otherwise take a bad feel, and could not be moistened without difficulty. On the contrary, when well drained, they are rinsed moderately in running water.

In aluming very little alum is fixed upon the silk, and the small quantity that is thrown down depends on the rinsing, which is preferably done in calcareous water. In this case the bicarbonate of lime contributes to the precipitation of alumina upon the fiber.

The aluming of silks which have already undergone certain dyeing operations is conducted substantially in the same manner, but the bath may be slightly heated, and is generally thrown away after use.

Sulphate of alumina, which now plays a certain part in wool dyeing, has not hitherto proved very successful for silks.

The most active preparation of alumina, which may, perhaps, be called the mordant of the future, is a solution of gelatinous alumina in methylamine. This compound, which may be regarded as the aluminate of methylamine, if exposed to the air, loses its methylamine, which is very volatile, while an abundant deposit of alumina is left without fear of injuring the fiber.

Alum ought to be as neutral as possible; it yields, then, with decoction of logwood, a very intense reddish violet, while if the alum is acid the color turns to a yellowish brown.

M. W. Stein recommends the use of ultramarine as a test, as its color is destroyed by the least traces of sulphuric acid in the alum.

[It must be remembered, however, that different makes of ultramarine have different powers of resisting acids.]

If the dyer uses sulphate of alumina, a salt usually acid, he may easily take account of this excess of acid by washing about 75 grains of the sample, pulverized in absolute alcohol, which seizes the acid, and leaves the pure sulphate, which is almost insoluble in alcohol. It is then easy to determine the quantity of free acid in the alcohol by diluting it with water and submitting it to an ordinary acidimetrical operation.

[If any of our friends are disposed to make use of this test, we should advise them to dry the samples of sulphate of alumina first at a heat not exceeding 212° Fahr. Otherwise, in case of damp samples, the alcohol would be considerably diluted by the water present.]—*Chemical Review.*

THE WAX OF FICUS GUMMIFLUA.

By F. KESSEL.

THIS wax is a chocolate colored mass, which softens when heated and melts between 60° and 70°. Boiling water extracts a considerable quantity of a brown coloring matter, leaving the wax nearly white. Boiling alcohol dissolves considerable quantities of the wax, which on cooling are redeposited in a white mass resembling cauliflower. By treatment with ether the wax may be separated into two bodies more or less soluble, the former having the composition $C_{18}H_{36}O$, and the latter, $C_{27}H_{54}O$.—*Ber. der Deutsch. Chem. Gesell.*

[Continued from SUPPLEMENT No. 180.]

RECENT SCIENCE.

NEW PRODUCTS FROM BEETS.

As the preceding notices have referred to theoretical rather than to practical subjects, it may be well to introduce an example of the value of applied science, to which attention has recently been directed by Professor Roscoe.* Modern chemistry nowhere shows to more advantage than in the utilization of waste products. Scarcely anything nowadays is allowed to run to waste. The residual substances and the by-products obtained in manufacturing operations are carefully studied by the chemist, and his studies are occasionally rewarded by the establishment of a new industry. Such has recently been the case with M. Camille Vincent, of Paris.

Since the beginning of this century the beetroot has been employed on the Continent for the preparation of sugar. Originating in Germany, the manufacture soon extended to France, where it was especially fostered by the first Napoleon in pursuance of his famous "continental system," which excluded, as far as possible, all colonial products from the French markets. To so great a magnitude has the manufacture now attained that it is said about 700,000 tons of beetroot sugar are annually manufactured in Europe.

To obtain sugar from this source, the cleansed beet is crushed, and the juice extracted by expression. This juice, having been refined and filtered, is boiled and allowed to crystallize. A sugar, identical with the ordinary product of the sugar-cane, is thus separated, while a thick, uncrystallizable sirup remains behind: this is, of course, the treacle or molasses. Beet molasses are used for sweetening purposes, or sometimes as fodder for cattle, but they are more generally employed for the preparation of a coarse spirit. To procure this spirit, the molasses, after having been diluted and acidulated, are subjected to fermentation and then to distillation. It is estimated that 100 kilogrammes of the molasses will yield about forty liters of spirit. After the distillation is completed there remains in the retort a liquid know as *vinasse*. This spent wash was formerly wasted, but it was shown many years ago by M. Dubrunfaut that it contains salts of potash. Instead, therefore, of being thrown away, it has of late years been utilized as a source of these valuable salts. The vinasse is evaporated to dryness and calcined, when it yields an ash termed *salin*, rich in compounds of potassium. The composition of the ash varies with the character of the soil on which it has been raised. By this process about 2,000 tons of carbonate of potash are annually produced from the French distilleries.

It might fairly be supposed that chemistry had now done its utmost to utilize the products of the beet, having obtained from it successively the sugar, the spirit, and the potash. Recent researches, however, have shown that the chemist has by no means exhausted his skill in that direction.

In the residual liquor from the distillation of the molasses there is not only the alkaline matter of the beet juice, which the root has extracted from the soil, but there is also much organic matter, some of which is nitrogenous. During the calcination of the dried vinasse this organic matter is decomposed, leaving a porous carbonaceous mass associated with the mineral residues. If the concentrated vinasse be subjected to destructive distillation in iron retorts, the volatile products of the decomposition may be secured, just as in the process of gas making. On passing these products through condensers, the condensable portions will be liquefied while the so-called permanent gases will pass outward, and may be utilized as fuel. The condensed products consist mainly of tarry and ammoniacal liquors; and in this respect they resemble the corresponding products obtained in the manufacture of coal gas, but the chemical composition of the products is not identical in the two cases. Thus the ammonia water obtained during the distillation of the vinasse ash contains, among other products, large quantities of the salts of trimethylamine. It is upon their presence that the new manufacture has been established.

Trimethylamine, a substance which was discovered nearly thirty years ago by Dr. Hofmann, belongs to the class of organic bodies known as "compound ammonias." Ammonia is a gaseous compound of nitrogen and hydrogen, which on solution in water forms common "spirit of hartshorn." It was shown in 1848, by M. Würtz, an eminent French chemist, that ammonia may have one of its hydrogen atoms replaced by an organic radical, like methyl. The new body thus formed is called methylamine. Pursuing this course, Dr. Hofman showed that a second atom of hydrogen might also be replaced by methyl, and thus a body termed dimethylamine was obtained. Finally the same chemist succeeded in replacing the third, or last, atom of hydrogen in ammonia by this radical, and in this way formed trimethylamine. This compound has hitherto been regarded as nothing more than a chemical rarity; but by the new French manufacture to which we are referring, it may now be prepared commercially on an enormous scale.

Although trimethylamine is at present of no industrial value, it is far otherwise with some of its products. Thus, M. Vincent has found that its hydrochlorate may be easily decomposed by heat into free ammonia (which is, of course, a useful product), free trimethylamine (which can be again converted into its hydrochlorate), and chloride of methyl. This chloride is a combustible gaseous body, easily condensed to a mobile liquid. Hitherto it has not been obtained in quantity, but M. Vincent can now prepare it to any extent, and, condensing it in strong wrought-iron cylinders, can transport it with ease and safety.

Chloride of methyl may be advantageously used in the preparation of some of the methylated colors, such as Hofmann's violets and the aniline greens. By the newly discovered source of methyl-chloride, the cost of preparation of these colors will be economized, since it can replace the more expensive iodide. But the most interesting application of chloride of methyl which has yet been proposed is that of a refrigerating agent. By the rapid evaporation of the condensed liquid a very great reduction of temperature is produced, and as the liquid is neither poisonous nor corrosive, it promises to become of much importance. Indeed, M. Vincent has constructed a freezing machine, in which, by the evaporation of the chloride of methyl, as low a temperature as —55° centigrade may be maintained; a temperature which is considerably below the freezing point of quicksilver. We have now, therefore, in our hands a new refrigerating agent, by means of which mercury may be frozen by the pound.

CHEMISTRY OF YEAST.

In the January number of this review, we gave an account

* "On a New Chemical Industry." A Lecture delivered at the Royal Institution, on February 21, 1879, by Professor Roscoe, LL.D., F.R.S. *Nature*, No. 467, February 27, p. 398.

of Naegeli's researches on the chemistry of yeast, and drew attention to the important discovery, in that fungus, of substances usually supposed to be confined to animals. Since that article was written, a paper has appeared by Mr. Sydney Vines,* in which similar results are arrived at for one of the higher flowering plants.

The seeds of many plants contain, in the cells, either of the endosperm or of the cotyledons, grains of a proteinaceous substance, usually wholly or partially soluble in water, and known as aleurone grains. These act as a store of proteid food material, as starch grains and oil globules are stores of hydrocarbons. Mr. Vines has made a careful microchemical examination of these granules in the blue lupin (*Lupinus varius*), and has obtained many important results, some new, others confirmatory of the observations of Weyl, a former writer on the same subject.

An extract of the seeds in a solution of common salt was found to contain two proteids belonging to the group of globulins, and hitherto known to occur only in animals: *myosin*, a constituent of dead muscle; and *vitellin*, a constituent of the yolk of egg. These two substances—vegetable myosin and vegetable vitellin—were found to have altogether similar reactions to the animal substances of the same name. An aqueous extract of the seeds contained another proteid, having all the properties of peptone, and agreeing very nearly with the *a* peptone of Meissner or hemialbumose of Kühne, an easily decomposable peptone formed by the action of gastric or pancreatic juice on proteids.

CHLOROPHYL-CONTAINING ANIMALS.

The above results show how nearly the chemical processes of plants approach to those of animals. The converse is shown, in an even more striking manner, by Mr. P. Geddes' researches on the chlorophyl-containing Planarians.†

Chlorophyl occurs in animals belonging to very diverse groups; it is found in certain *Infusoria*, in one of the freshwater sponges, in the common *Hydra viridis*, and in the sea anemone, *Anthea cereus*, in three species of Planarians, in the tubeworm, *Chætopterus Valenciennesii*, in *Bonellia viridis*; and in an isopod, *Idotea viridis*. The fact that the green grains contained in these animals are chlorophyl, as far as their chemical and spectroscopical characters are concerned, has been proved by Cohn, Ray, Lankester, and others; but it has never hitherto been certainly shown that they are physiologically identical with plant chlorophyl; that is, that they have the power of decomposing carbonic acid.

Mr. Geddes has now succeeded in proving this point as far as the Planarians are concerned. He found that if a number of specimens were placed in water and exposed to direct sunlight, they gave off bubbles of gas, and that this gas contained from 45 to 55 per cent. of oxygen—enough to rekindle a glowing taper. The habits of the animals are quite such as would be expected from this: they are found on the sand by the sea shore, exposed to full sunlight, and covered by only a few centimeters of water; in an aquarium they always seek the light, and they were found, in almost every instance, to live far longer if exposed to light than if kept in the dark.

Their chlorophyl was, like that of plants, dissolved out by alcohol; and an examination of the animals thus coagulated and bleached furnished a further link between the physiological processes of these animals and those of plants. Mr. Geddes found that the addition of iodine to an aqueous extract of the alcohol specimens gave the characteristic blue color, disappearing by heat and reappearing on cooling, which indicates the presence of the distinctively vegetable substance starch.

The starch occurred in the form of minute but definite granules; the chlorophyl, on the other hand, was evenly diffused through the cells, as in many of the lower *Algæ*, not aggregated into grains as in the higher plants.

The significance of these results is well summed up by the author: "As the *Drosera*, *Dionæa*, etc., which have attracted so much attention of late years, have received the striking name of carnivorous plants, these Planarians may not unfairly be called vegetating animals, for the one case is the precise reciprocal of the other. Not only does the *Dionæa* imitate the Carnivorous animal, and the *Convoluta*, the ordinary green plant, but each tends to lose its own normal character. The tiny root and the half-blanched leaves of *Pinguicula* are paralleled by the absence of a distinct alimentary canal and the abstemious habits of the Planarian."

The group of *Turbellaria*, to which the Planarians belong, is one of the lowest among the *Metazoa*, or many-celled animals, and in many points of structure it forms a starting point from which a number of the higher groups diverge. In the lowest members of the class, *Convoluta* and *Schizoprora*, it has been shown by Metschnikoff, Uljanin, and others, that there is actually no alimentary canal, at any rate in the adult, but that the food taken in at the mouth is passed directly into the general parenchymatous tissue of which the substance of the body is composed, and there digested. The process of alimentation, therefore, in these *Turbellaria*, resembles, in its general features, that met with in *Infusoria*.

That the actual details of the process are similar in the two cases has recently been made out by Metschnikoff's researches of two genera of Turbellarians, *Mesostomum* and *Planaria*.‡ The main difference between the process of ingestion of nutriment in a Protozoon and in one of the higher animals is that, in the latter, the food is brought into a state of solution, while in the digestive cavity, by means of certain juices secreted in the cells lining its walls, and is then absorbed by those cells, by a passive process of diffusion, while in the Protozoon the food particles are taken bodily into the substance of the one-celled organism and there assimilated.

In giving an account, in a former number of this Review,§ of Reichenbach's researches on the development of the crayfish, we called attention to that author's interesting observation that the cells of the developing embryo engulf the yolk-spheres which serve them as pabulum by surrounding them with pseudopodia, the cells of the embryo crustacean thus behaving like so many independent *Amœbæ*. The individual cells, or sponge particles, of the many-celled sponges take in their nutriment in the same active manner; but it is quite a surprise to find the comparatively complicated *Turbellaria* in a similar case.

One of the species examined by Metschnikoff, *Mesostomum Ehrenbergii*, feeds largely on the freshwater worm *Nais*. When observed about an hour after a meal, the cavity of the alimentary canal contained nothing but the cuticle of the worm and its setæ, but in the interior of the amœboid cells lining the canal were found the whole of its soft parts, certain characteristic pigmented cells being especially noticeable. Sometimes even the setæ and fragments of other hard parts were found to have been taken in by the alimentary cells. The case was rendered even more striking by feeding the worms on carmine before allowing them to be devoured by the *Mesostomum*. In this case the digestive cells of the latter were seen, after an interval, to be crammed with particles of the coloring matter.

Similar experiments were tried with *Planaria lactea* and *P. polychroa*, which were fed upon blood containing finely divided carmine or indigo. In this case the cavity of the alimentary canal completely vanished during digestion, its lining cells swelling up considerably beyond their original size, and being filled with immense numbers of blood-corpuscles and granules of coloring matter.

NUCLEI OF CELLS.

Within the last few years the attention of many accomplished histologists has been directed to the difficult problem of determining the exact structure of the nuclei of cells, a subject of which next to nothing was known up to the commencement of the present decade. In Professor Stricker's article "On the General Character of Cells," published in 1869,* the question is discussed as to whether the nucleus is vesicular or not, and it is stated that in cell division the nucleus "elongates, becomes hourglass shaped, and ultimately constricted into two segments;" but nothing is said as to its minute structure beyond the fact of the presence of one or more nucleoli.

Recently, however, the question of the exact structure of nuclei, and of the precise part played by them in cell division, has been taken up by Auerbach, Strasburger, Van Beneden, and Oscar Hertwig, of whose researches an account was written about three years since by Mr. Priestley,† as well as by Klein, Flemming, and many others. Their observations seem to show that the cell nucleus—the length of which, it must be remembered, does not average more than 1-2,000th of an inch—is inclosed in a definite membrane, and consists of a more or less complicated network of delicate protoplasmic filaments, the intra-nuclear network, embedded in a pale, apparently structureless ground substance. According to Dr. Klein,‡ this network is continuous through minute apertures at the poles of the nuclear membrane, with an intra-cellular network in the substance of the cell itself, and, in the case of ciliated cells, the cilia are continuous with the intra-cellular network. So that Klein's view of the nucleus is that it consists of a central, more complicated portion of the general intra-cellular network, shut off from the rest by the formation of an investing membrane. Professor Flemming§ denies the existence of a connection between the intra-nuclear and the intra-cellular networks; and Herr Schleicher‖ considers that in cartilage cells, at any rate, there is not even a true network in the nucleus, but merely a number of independent filaments, rods, and granules, which put on the appearance of one.

There is also a difference of opinion with regard to the nucleoli, the usually highly refracting bodies which occur in nuclei, one or more to each, and which, prior to the observations we are recording, were the only points of structure made out in nuclei beyond the possible existence of a membrane. Professor Flemming believes that these have a real existence—that they are, in fact, granules lying in the meshes of the intra-nuclear network, and of a different substance from the latter. Dr. Klein, on the other hand, considers them to be mere local thickenings of filaments, or to result from the shrinking and fusion of a part of the network. The latter view is favored by their great inconstancy, and by the fact that they are often by no means evident in the fresh condition.

The foregoing observations all apply to nuclei in the quiescent condition; the nucleus of dividing cells has also been the subject of careful study, both in animal and vegetable cells. The main steps in the process, as given by Auerbach, Strasburger, and other earlier observers, are shortly as follows.¶ The nucleus elongates until it has assumed a spindle shape, and exhibits a longitudinal striation, the striæ converging at its extremities, and gradually diverging as they reach its equator, where is finally seen a disk-like structure placed at right angles to the long axis of the nucleus, and composed of short rods continuous with the striæ. This nuclear disk splits into two, its rods becoming drawn out in the center, while their still thickened ends travel outward along the striæ of the nucleus, until they reach one of its poles; in this way there is formed at each extremity of the spindle an accumulation of substance. These two accumulations, assuming gradually a more defined shape, become the nuclei of the two daughter cells into which the mother cell is dividing. A second disk then appears in the equator of the nucleus by central thickenings of its striæ; this cell-disk also splits into two, the plane of junction between its two segments marking the division plane between the new daughter cells. Often, during this process, the protoplasm of the cell takes on a radiate arrangement round the poles of the spindle, producing the appearance of a sort of double star.

These appearances in the dividing nucleus were made out in various vegetable cells and in the ova of many animals; the relation between them and those described above in the quiescent nucleus seems by no means clear, but special attention has been directed to this point by Flemming,** and by Schleicher,†† whose results in the case of the nuclei of epithelium, cartilage, etc., show a very close agreement in the main features of the process, although they differ considerably in detail. Both have directly observed the division process in living cells. Flemming has also availed himself extensively of various preservative and staining fluids, and it is in his very beautiful figures of these hardened and colored nuclei that the chief discrepancies are observable between his results and those of Schleicher. It will be seen that their results differ in some particulars from those of former observers.

The first step, preparatory to cleavage, is a disintegration of the nuclear membrane, fragments of which seem to become mingled with the other constituents of the nucleus. Then the filaments of the intra-nuclear network (or the rods, filaments, and granules, according to Schleicher) assume a very lively condition of motility, and go through an extraordinary series of arrangements and re-arrangements, producing figures now star-like, now wreath-like, now more irregular; but, according to Flemming, taking place in a definite order. The filaments then assume an almost parallel disposition—forming the so-called cell plate or equatorial plate—and then, diverging somewhat toward the equator of the nucleus, and converging at its two poles, produce the spindle form already described. The remarkable series of bacteroid movements by which those changes are produced is called by Schleicher "karyokinesis;" the process is so vigorous that the nucleus of cartilage cells, freed from its membrane, travels now to one pole of the cell, now to the other, now contracts and now enlarges to such an extent as to fill up nearly the whole cell.

After the assumption of the spindle form, the formation of the new nuclei begins, according to Schleicher, by a fusion of the converging ends of the filaments—the ends, that is, turned toward the poles of the spindle—while at the same time a separation of the fibers takes place along its equatorial plane. The young nuclei are, therefore, tolerably solid at first, but afterward split up into filaments, rods, and granules, some of which, at the periphery of the nucleus, curve round, unite with one another, and form a new nuclear membrane. According to Flemming's observations, which are partly borne out by Schleicher's, the daughter nuclei go through the same series of changes, but in inverse order, as were undergone by the mother nucleus in the various stages of karyokinesis.

There is one observation of Schleicher's which has an interesting bearing on Klein's view of the connection between the intra-cellular and intra-nuclear networks. He observed, in certain cases, filaments in the cell protoplasm which seemed to become continuous with the nuclear filaments during karyokinesis. Moreover, these cell filaments had an intimate connection with the capsule of the cell, leading to the opinion that they were split off from its internal surface by a process of delamination.

MATURATION AND IMPREGNATION OF THE ANIMAL OVUM.

Closely connected with these researches on nuclei are the numerous observations lately made on the phenomena which accompany the maturation and impregnation of the animal ovum. The ripe egg consists essentially, in all animals, of a vitellus or protoplasmic cell body, containing usually a greater or less quantity of food material in the shape of yolk granules, of a surrounding vitelline membrane or cell envelope, and of an inclosed cell nucleus or germinal vesicle, which latter is contained, like the nuclei, of which we have been speaking, in a distinct membrane, and contains one or more nucleoli or germinal spots, as well as a delicate network of protoplasmic filaments.

It has long been known that, prior to impregnation, the germinal vesicle undergoes important changes, but what is the nature of these, what the precise nature of the influence exerted by the spermatozoon or spermatozoa effecting impregnation, and what the mode of origin of the nucleus of the impregnated egg or first segmentation nucleus, has only recently been made out. The observers to whom we owe this important and difficult piece of work are chiefly Auerbach, Strasburger, Van Beneden, Bütschli, Oscar Hertwig, and Fol; a *resume* of their observations was published last year by Mr. Balfour,* who has himself made important contributions to the subject. Since the publication of this *resume*, the most noteworthy papers are these of Oscar Hertwig† and of Calberla,‡ the former of whom has studied the eggs of mollusks, worms, and echinoderms, the latter those of the lamprey.

With regard to the fate of the germinal vesicle, the most complete observations are those of Hertwig, whose figures, drawn very largely from the living objects, seem to leave no doubt about the main steps of the process. The vesicle travels from the center to the surface of the egg, its membrane undergoes disintegration, its nucleolus disappears as such, and it is converted into a spindle-shaped body, with the usual delicate striations, and with the star-like radiation of granules from its poles. A prominence is then formed on the surface of the vitellus, into which one end of the spindle passes; the spindle itself undergoes division in the usual manner, one segment being left in the egg, the other in the prominence, which then separates itself as the first of the "polar bodies" or "directive corpuscles"—small structures now proved by their mode of formation to be true cells, which lie between the egg itself and the vitelline membrane, occupying a constant position throughout the early stages of development. The likeness between this process and those mentioned above as characteristic of ordinary cell division will be at once apparent.

The next step is the formation, in the same manner, of a second polar body, the spindle-shaped nucleus, as before, dividing into two, one for the polar cell, the other for the egg itself. The latter portion undergoes a change, being converted into a more or less rounded body with a radial striation of granules around it, the female pronucleus (Eikern), which gradually travels to the center of the egg. In the meantime, a (in most cases) single spermatozoon has made its way through the vitelline membrane into the vitellus, and, its tail being lost or fused with the vitellus, its head has been converted into a body closely resembling the female pronucleus, and known as the male pronucleus (Spermakern). This travels toward the female pronucleus, and completely fuses with it, the first segmentation nucleus being the result of the fusion.

As Mr. Balfour remarks, the head of a spermatozoon is, in all probability, the modified nucleus of a spermatic cell, so that the process of impregnation consists in the conjugation of two nuclei. Moreover, both ova and spermatic cells are developed in the embryo from certain cells of undetermined sex known as primitive ova,§ so that this conjugation is a union of morphologically identical structures.

INTELLIGENCE of the most gratifying character comes from the iron manufacturing districts of the Ohio Valley, showing a general revival of the industry. Numerous blast furnaces and rolling mills are resuming operations, and the outlook is full of promise to all.

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* "On the Chemical Composition of Aleurone Grains." *Proc. Roy. Soc.*, vol. xxviii., No. 191, December 10, 1878.

† "Sur la fonction de la chlorophylle avec les Planaires vertes." *Comptes Rendus*, December 30, 1878, and "Observations on the Physiology and Histology of *Convoluta Schultzii*." Read before the Royal Society on March 27.

‡ "Ueber die Verdauungsorgane einiger Susswasser-Turbellarian." *Zoologischer Anzeiger*, December 30, 1878.

§ *Nineteenth Century*, December, 1877.

* Stricker's "Human and Comparative Histology," vol. I. English translation by H. Power. 1870.

† "Recent Researches on the Nuclei of Animal and Vegetable Cells, and especially of Ova." *Quart. Journ. of Microscopical Science*, vol. xvi., p. 191. 1876.

‡ "Observations on the Structure of Cells and Nuclei." *Quart. Journ. of Microscopical Science*, July, 1878, and April, 1879.

§ "Beitrage zur Kenntniss der Zelle und ihrer Lebenserscheinungen." *Archiv f. micr. Anat.*, xvi. Band, 2 Heft, December, 1878.

‖ "Die Knorpelzelltheilung." *Archiv f. micr. Anat.*, xvi. Band, 2 Heft, December, 1878.

¶ See Priestley, *loc. cit.*

** *Loc. cit.* †† *Loc. cit.*

* "On the Phenomena accompanying the Maturation and Impregnation of the Ovum." *Quart. Journ. of Microscopical Science*, April, 1878.

† "Beiträge zur Kenntniss der Bildung, Befruchtung, und Theilung des thierischen Eies. Dritter Theil, I. Abschnitt." *Morphol. Jahrb.*, iv. Bd., 1 Heft; and "II. Abschnitt" *ib.* 2 Heft, 1878.

‡ "Der Befruchtungsvorgang beim Ei von Petromyzon Planeri." *Zeits. f. wiss. Zool.*, xxx. Band, 3 Heft, 1878.

§ See Balfour "On the Structure and Development of the Vertebrate Ovary." *Quart. Journ. of Microscopical Science*, October, 1878.

PROF. CROOKES' REMARKABLE DISCOVERIES CONCERNING MOLECULES.

THE conversazione which was lately given at Burlington House by the President and Council of the Royal Society, London, was an exceedingly brilliant and successful one, and was in no way inferior to any that has gone before it, whether for the number of distinguished guests who availed themselves of the invitation, or for the importance and scientific interest of the objects collected for their pleasure and instruction.

As is usual on these occasions, Mr. William Crookes, F.R.S., contributed very largely to the success of the evening by his superb display of experimental illustrations of a branch of physical research which he has made his own quite as much as his celebrated investigations connected with repulsion resulting from radiation, which gave it birth. This new research, the investigation of certain phenomena connected with molecular physics in high vacua, Mr. Crookes first brought before the Royal Society in the month of December last. We pointed out that Mr. Crookes' experiments tended to show that there exists in nature a fourth and higher condition of matter as much more ethereal than the gaseous condition as the gaseous state is higher than the liquid. Such experiments of Mr. Crookes as have produced the radiometer, the otheoscope, and the exceedingly beautiful series of experimental researches upon which he is still engaged, would have been impossible even to have been commenced before the days of the Sprengel pump, but Mr. C. H. Gimingham, by his great and important improvements in that instrument, has placed in the hands of the physicist an instrument by which vacua can be produced, compared with which the highest exhaustions effected by the old-fashioned air pump must be regarded almost as high pressures. The internal pressures of the gaseous residues within Mr. Crookes' apparatus are measured in millionths of an atmosphere, and in many of his more recent researches he has had to deal with vacua whose pressure is measured in small fractions of but one millionth of an atmosphere.

We have described Mr. Crookes' experiments which led to the discovery that the dark space around the negative pole within a vacuum tube, and separating it from the luminous nebulous glow, is a field of molecular activity which appears to bear a very close relationship to, if it is not identical with, the field of molecular disturbance in front of the vanes of a radiometer, and by which its mechanical motion is produced. Fig. 1 represents a small apparatus in which

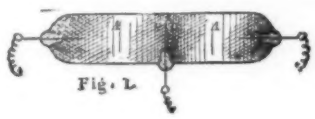

Fig. 1.

this dark space is very beautifully shown. A A is a cylindrical vacuum tube having an aluminum wire sealed into each end at C C, and carrying across its middle diameter a circular disk, B, of aluminum, which is of slightly smaller diameter than the tube, and is connected by the wire shown in the sketch to the negative pole of an induction coil, the two wires, C C, both being connected to the positive pole. When this exhausted tube is placed in electrical communication with the induction coil, the tube is filled right and left of the disk, B, with a beautiful nebulous violet glow, with the exception of a space on each side of the disk shown at D D; this is the dark space around the negative pole, and its width from the disk increases as the exhaustion is continued, and if this be carried on sufficiently it will spread to the two ends of the tube, driving the luminous glow before it. When the negative pole is free to move like the vane of a radiometer and one of its faces is insulated by a film of mica, no motion takes place until the exhaustion is such that the dark space reaches the surrounding glass envelope or tube, and the moment that this occurs rotation commences. The electrical radiometer embodies this principle and consists of a radiometer carrying flat aluminum vanes, each of which is covered on one face with a thin disk of mica; this fly is in metallic communication through its center with an induction coil of which it forms the negative electrode. The positive electrode can be inserted in any other part of the bulb, its position in no way affecting the result. When the induction coil is placed in circuit with this apparatus the luminous glow gradually separates from the metallic surface of the vanes under exhaustion, leaving a dark space, which widens as the exhaustion is continued until the inner surface of the envelope or bulb is reached, when rotation of the fly immediately commences. This little instrument, which was shown by Mr. Crookes at the soiree, besides illustrating the very beautiful phenomena for which it was designed, exhibited optical effects: thus, whenever the number of vibrations of the contact-maker bore an exact proportion to the speed of rotation of the fly, it appeared to be standing perfectly still, although really rotating at a high velocity, the number of vanes seen depending upon what the proportion was between the two speeds; these effects, which are familiar to those who have played with the color top, especially when illuminated by electrical discharges, were especially strikingly illustrated in the instrument to which we refer, for the reason that the rotating object was at the same time the source of light by which it was itself illuminated. Similar effects were produced with a radiometer, the vanes of which were aluminum cups.

We have on a former occasion referred to the very beautiful phosphorescent effects produced by molecular impact in tubes, in which the exhaustion is carried to high degree. With the ordinary soft German glass of which Mr. Crookes' tubes are generally constructed, this phosphorescence displays itself as a beautiful greenish yellow light spread over all those portions of the glass upon which the molecular stream impinges, and appears to be due to the sudden arrest of the gaseous molecules which are projected at a high velocity against the inner surface of the glass by a series of bombardment, their motion being suddenly arrested, and their energy being thereby converted into luminous effects.

We have also pointed out that if any body, whether optically transparent or not, be interposed between the negative pole and the surface which is rendered phosphorescent by the molecular bombardment, a sharply-defined shadow of the object is cast upon the glass—that is to say, the interposed body acts as a shield or screen, arresting the streams of molecules projected in straight lines from the negative pole, and allowing only those portions of the glass to be subjected to molecular compact which are not screened from the negative pole by the object placed in the molecular path. This beautiful effect was illustrated by Mr. Crookes with the very simple little apparatus shown in Fig. 2, in which A A is a pear-shaped vacuum tube or bulb, having a flat aluminum disk, B, fixed at one end; in connection with the negative electrode, C, of an induction coil, is a Maltese cross cut out of thin aluminum foil fixed to hinged support, so that it can be set up or laid down flat against the lower side of the tube by slightly shaking the apparatus. For convenience this is made the positive electrode, but there is no other necessity for the cross, C, being electrically connected with the coil, as its only object is to present a screen or obstruction to the molecular streams projected from the disk, B, upon the end of the bulb. When this cross is lying flat at the bottom of the tube it offers no obstruction to the molecules in their path, and the larger end of the tube glows with the beautiful green "phosphorescent"

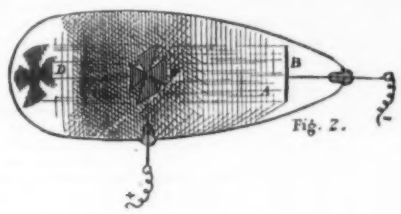

Fig. 2.

light of molecular impact, but the moment that the cross is set up so as to stand in the path of the stream, a perfectly sharply-defined shadow is cast at D, producing a non-luminous transparent cross upon a green semi-opaque and luminous ground, indicating by its sharpness that the molecular streams are projected in perfectly straight lines, and in no way curve round an object or overlap one another. If, however, a magnet be brought near to the apparatus, these streams are deflected up or down, right or left, according to the position of the poles with respect to the tube, and in that case their paths become curved. This is shown by the shifting about of the shadow at the end of the tube, its position altering with every movement of the magnet, but its sharp definition remains the same. This magnetic deflection of the trajectory of molecules may be compared to the effect of gravitation upon the path of a cannon ball projected from a gun in a horizontal direction; if the action of gravitation were eliminated the trajectory of the shot would be a horizontal straight line, but the action of gravitation, combined with the constantly diminishing inertia of the ball, gives curvature to its trajectory.

As the molecular streams are projected from the negative pole in straight lines which start in a direction normal to the surface of that pole, it is perfectly easy by giving to that pole a plane or a curved surface to concentrate the molecular streams to a focus, to cause them to diverge or to move in parallel straight lines, and Mr. Crookes has made a large number of experiments with cup-shaped poles by which effects of concentration are produced; one of the most striking of this most superb series of experiments is one which, although we have described it before, we cannot omit on the present occasion, as it formed one of the principal demonstrations shown by Mr. Crookes the other evening. This experiment is the production of intense heat by the concentration of streams of molecules upon a focal point, which thus receives the combined impacts of the great majority of the molecules projected from the negative pole. The apparatus by which Mr. Crookes demonstrated this beautiful phenomenon is shown in Fig. 3. A negative

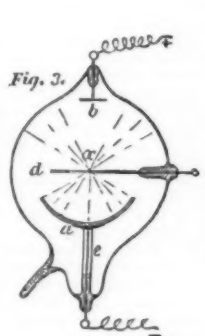

Fig. 3.

Fig. 4.

Fig. 5.

pole, *a*, which is in the form of a large cup of spherical curvature, is fixed by the stem, *c*, at the lower portion of a glass bulb similar to that of a radiometer, the positive pole, *b*, being fixed at the top of the bulb. At *d* is placed a narrow strip of platinum foil, at such a distance from the cup-shaped electrode, *a*, as to pass through its center of curvature, *x*. From what has already been said it will be seen that when the two electrodes are connected to the poles of an induction coil, a concentrated fire of molecular projectiles is received upon the platinum strip, *d*, at the point, *x*, and the effect is to cause this point at once to glow with incandescence, and so great is the heat produced that, unless the action be promptly stopped, the platinum would be fused and the instrument destroyed.

This little apparatus shows in a very beautiful manner the deflection of the molecular streams under the influence of magnetism, for if a magnet be manipulated near the bulb the incandescent spot may be made to travel to and fro along the platinum strip or directed to any one point, or by deflecting it to the right or left of the strip it may be thrown off it altogether, no incandescence being produced.

For the exhibition of the "phosphorescent" effects of molecular impact, the tube shown in Fig. 4 is perhaps the most striking; it consists of a cylindrical tube, A A, having a semi-cylindrical negative pole, C, at its lower end. Above this is fixed a phosphorescent screen or plate, B B, coated with M. Becquerel's luminous sulphide of calcium. When this tube is placed in circuit with the induction coil the field of molecular disturbance is splendidly marked out by the phosphorescent figure on the screen, the planes of molecular streams first being brought to a focus, which is shown by the small area and greater brilliancy of their point of intersection, and then, diverging again, producing a surface of illumination diminishing in intensity as the distance from the focus is increased. The light emitted from this tube was strong enough to enable objects to be seen in the otherwise dark room, and it was perfectly possible to read small print when the paper was held near the illuminated tube. For optical beauty, however, the tube shown in Fig. 5 was perhaps the most remarkable of the whole series. In construction it differed from the last piece of apparatus only in being without the flat phosphorescent screen, the beautiful effects being produced by the green phosphorescence of the tube itself. A A is a cylindrical vacuum having a semi-cylindrical electrode at C, which is connected with the negative pole of an induction coil. When the circuit of the coil is completed, the beautiful green light of molecular impact that is produced, and the planes of disturbance intersecting in a sharply-defined focal line crossing and impinging on the opposite cylindrical surfaces of the glass envelope so as to form ellipses of brilliant green phosphorescence, can hardly be imagined without being seen, and the action of the cylindrical electrode being to condense the molecular streams in one plane, but not in others, produces the effect of two flat surfaces of elliptic boundary crossing one another, the space between and above them being filled with a beautiful but less intense illumination.

Fig. 6 represents an apparatus which illustrates in a very

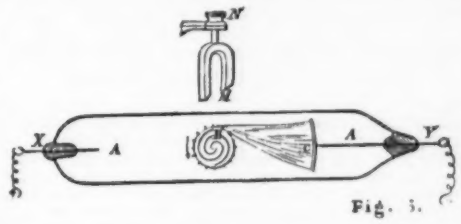

Fig. 6.

beautiful manner the four principal phenomena connected with molecular physics in high vacua—first, the green phosphorescent light of molecular impact; second, the projection of molecular shadows; third, the magnetic deflection of the trajectory of molecules; and fourth, the mechanical action of the gaseous molecules projected from the negative pole. A A is a vacuum tube carrying at C a concave circular negative electrode whose center of curvature falls about the middle of the tube; at this spot a light wheel, B, is pivoted upon a horizontal axis. This wheel, which is shown enlarged in Fig. 7, consists of a disk of thin mica carrying

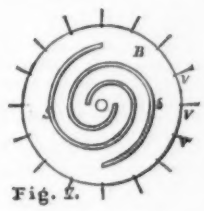

Fig. 7.

around its periphery a number of equidistant radial vanes of aluminum, whose planes are perpendicular to the plane of the disk. The wheel, therefore, bears some resemblance to a water-wheel. When this tube is placed in connection with the induction coil a concentrated stream of molecules is projected on to the wheel, which is pivoted at or near the focus of the electrode, C, and falling neither above nor below its axis, no rotation takes place; but when a small magnet, M, which can be turned on a vertical axis, is put into such a position that the cone of molecular rays is bent upward, the focus of impact is received by the upper vanes, and the wheel rotates with great rapidity in the direction indicated by the arrow. On, however, reversing the position of the magnet, the molecular streams are repelled, and, falling on the lower vanes of the wheel, cause it to revolve in the opposite direction. To express the effect in a very few words, one position of the magnet converts the movement into an over-shot wheel, another position into an under-shot wheel, while the intermediate position, by allowing the stream to strike the wheel equally above and below its center, has no motive effect upon it, and consequently it remains at rest. In order to make the rotation of the wheel and its direction apparent to an audience, two opaque spiral bands, S S, were painted on the mica disk, and an image of the wheel and bands was projected as a transparency on the screen. By this simple contrivance the direction of rotation, even at the highest speeds, was visible, by the apparent inward or outward motion of the image of the spiral bands. This very beautiful little instrument, as well as all the others connected with this valuable research, was made by Mr. C. H. Gimingham, whose unparalleled skill appears to increase with the difficulty and delicacy of the feats which he sets himself to accomplish.

Some of the most striking of Mr. Crookes' experiments are the illustrations of the brilliant effects by the immersion of naturally phosphorescent substances in the molecular stream. Fig. 8, A A, represents a tube, in which a diamond,

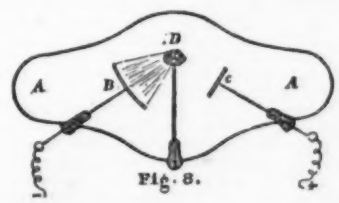

Fig. 8.

D, is supported on a glass stem in the focus of a concave electrode, B, the other electrode, C, being connected with the positive pole of an induction coil. When this is in circuit the diamond glows with a brilliant colored light of sufficient intensity to light a small room, and to illuminate objects on the same table as well as if an ordinary candle were substituted for the phosphorescent diamond. Mr. Crookes has observed that different species of diamonds shine with different colored phosphorescent light. Thus most of the African diamonds emit a blue phosphorescence, while those from other parts of the world shine, when under the same conditions, with the following colors: blue, pale blue, orange, red, green, and pale green. Fig. 9 is a tube for illustrating the phosphorescence of other substances and precious stones; in one of these tubes Mr. Crookes had diamonds, in another rubies, and in another white precipi-

tated alumina. The effects of these different bodies in the molecular stream were very splendid and interesting; with a number of diamonds, each glowed with its own characteristic phosphorescence, producing a beautiful combination of color; while the rubies, whether pale or deep in color, all assumed the deep "pigeon" red characteristic of a fine ruby.

FIG. 9.

The white precipitated alumina, which is almost identical with the ruby in chemical composition, when under the influence of the molecular stream projected from the negative electrode, B, Fig. 9, assumed the bright red color characteristic of the ruby, although not the slightest trace of color could be observed under ordinary conditions.

PHYSICAL SOCIETY, LONDON, APRIL 26, 1879.

Prof. W. G. ADAMS, President, in the Chair.

MR. C. V. BOYS gave an account of some experiments made by Dr. Guthrie and himself on the subject of Arago's rotation. The experiments were begun with a view to determine if the drag on a copper disk when a magnet is made to revolve beneath it, or on the magnet if the disk is made to revolve above it, could be made use of for determining the velocity of running machinery. They made the magnet revolve, and obtained the angle of deflection of a disk suspended by a torsion thread (the hair spring of a watch). They found, as Snow Harris and others found before, that, other things being equal, the drag is directly proportional to the speed, so that if the torsion of the thread could be relied on, and the strength of the magnet did not change, a perfect velocimeter could be constructed. They consider that this method is better than observing the deflection of a magnet over a revolving disk, as in this case they are limited to less than a right angle, and changes in the absolute magnetism of the earth would affect the results. They also determined the effect of change of distance, thickness, diameter, and nature of the disk, etc., their results agreeing with those of former experimenters. They observed that the effect of concentric circular cuts was far greater than that of even many radial cuts, and that when radial sectors were entirely separated from each other the effect was much less than when these were united at the center. They then experimented on liquids by suspending a sphere or cylinder of the liquid between the poles of a revolving electro-magnet, and succeeded in getting a decided and measurable effect. The importance of this is very great, for they have thus a means of determining the conductivity of liquid electrolytes by currents induced in the liquid without the use of electrodes and without polarization.

Dr. Guthrie stated that as the push on the liquid is directly proportional to the current quantity they hope to measure the conductivities of liquids, and connect these to the conductivity of solids through the intervention of mercury.

In reply to Prof. Adams,

Mr. Boys said that the angle of deflection of the conductor had proved to be proportional to its conductivity.

Dr. O. J. Lodge suggested that the conductivity of the disks used in these experiments should be determined by plotting out the equipotential surfaces.

Dr. Sylvanus Thompson recommended trying conducting jellies in these experiments, and

Dr. Guthrie replied that such were being prepared for trial, including the permanent jelly made by dissolving gelatine in anhydrous glycerine at 100°.

Prof. Sylvanus Thompson then communicated five laboratory notes from University College, Bristol. The first related to the source of sound in the Bell telephone receiver. Two theories are now being discussed as to this effect: the molar theory regards the motion of the diaphragm in mass as the source of sound; the molecular theory finds it in the molecular motions of the magnetic core of the instrument. Prof. Thompson applied his method of getting magnetic curves with iron filings dusted on gummed glass to this problem. He found that when no currents passed in the telephone the magnetic lines springing from the pole of the magnet are gathered together on the diaphragm opposite, over a central region which is magnetized lamellarly, or like a magnetic shell. The rim of the plate beyond this region is, however, magnetized radially, and between these two zones there is a neutral circle. It was remarkable, too, that the lines of force touching the plate were bent back around the circle, forming a kind of valley. When the current passed in the coil in a direction so as to re-enforce the magnetism, the lines are gathered more closely on the central region of the plate. If the current diminishes the magnetism, the lines are, on the other hand, repelled from the plate. The neutral line is also altered. In the first case it shrinks in size, in the second it expands. A small thick disk is wholly magnetized lamellarly; a disk entirely magnetized radially becomes slightly conical in shape. In the actual telephone the disk is flat at the middle and conical at the edges. As the neutral ring shifts the diaphragm will assume new nodal lines. Dr. Thompson concludes that the molecular theory is not, therefore, necessary to account for the speech of the telephone, although it may assist. As confirming this view he found that with iron rings round a cardboard diaphragm and an iron center piece the enunciation was good, though the timbre was altered, whereas with radial pieces of iron on the cardboard the timbre was good, but the enunciation bad. In reply to Prof. Adams, Dr. Thompson said that the stronger the magnet the smaller the lamellarly magnetized space became, and that with a thicker disk the neutral ring was not so well marked.

Dr. Lodge suggested that the best place for the coil would be in the valley over the neutral ring, which was in an unstable condition.

Dr. Thompson next wrote on a saw blade with a magnet, and dusted iron filings on it, which arranged themselves so as to trace the writing. This is usually shown on a steel plate, but a saw retains the virtue for six or eight months. A modification of this experiment due to himself consisted in writing on the blade with one pole of a powerful battery, the other pole being connected to the end of the blade.

The third note recommended the use of fine steel fibers, got by breaking iron gauze of 32 meshes to the inch, instead of iron filings for exhibiting magnetic lines. The fourth note showed that the lines of force got by filings fixed on cards are magnetic, that of a magnet acting as a magnet. The fifth note explained that solid magnetic figures could be got by coating iron filings in shellac to make them light, and floating them in water, or by mixing filings in a soft paste of plaster of Paris, which could be cut into sections on hardening.

EXTINGUISHING FIRES IN TAR DISTILLERIES, ETC.

By WATSON SMITH, F.C.S., F.I.C.

IT may not be known to all tar distillers that, in cases of fire on their premises, their best remedy usually lies close at hand, and this I will shortly endeavor to demonstrate from a basis of experience and fact. In the first place, however, it may be well to state that, as a rule, the tar distiller cannot get any fire insurance company to insure him, and it will, therefore, be seen that it is a matter of no light importance to be possessed of a ready and efficacious method of subduing the dread element when it breaks out beyond its due bounds. This remedy lies in the crude ammonia water, otherwise known as gas liquor, which almost every distiller works up conjointly with the tar, and if in some cases this be not so, it would be little encumbrance and well worth while to obtain and keep a stock of say 1,000 gallons of this gas liquor. The way in which I first became aware of the eminent extinguishing powers of gas liquors (now several years ago) is as follows:

A quantity of pitch was being run from a tar still into the pitch house, in which it is allowed to stand for some hours, to allow most of the noisome vapors to condense. By some means a flame came in contact with the vapors, an explosion occurred, and in a moment the roof was blown off, and the whole mass of molten pitch ablaze. Water, though thrown on it in quantity, seemed to avail nothing, and at last the supply accidentally ran short. Almost in despair, the pumps for raising the gas liquor into the ammonia stills were adjusted so as to deliver a jet of the liquor upon the fiercely burning pitch. The expedient acted like a charm, and the fire was quickly smothered.

I think there is considerable probability that, at the high temperatures attained in some fires, even the ammonia itself would be decomposed, with liberation of nitrogen and hydrogen gases, which, in addition to the volumes of carbon dioxide and steam, choke out the flame by simple displacement of air. It might be said that, if hydrogen and nitrogen be so liberated together, these, together with the H_2S also set free, would do much to neutralize the effect of the non-combustible gases. Not so, however, for let there be but enough incombustible gas present, as there is, to displace the air, or to choke by excessive dilution the little which may remain or diffuse, then the combustible gases present, thus for the time prevented from burning, would assist in smothering the flame (being non-supporters of combustion) with as good a will, if one may use the expression, as the incombustible ones.

I would strongly recommend every tar distiller, who also works up gas liquor, to so arrange his pumping gear that he may be able to throw jets of this liquor into any part of his yard or works where fire might break out and prove disastrous. If gas liquor be not worked, then I would advise that a stock of say 1,000 gallons be purchased. In a small works this might be advantageously stored in a tank or old boiler mounted on brickwork, at such an altitude as to give pressure enough to furnish a good jet for service below when required. Of course, this reservoir should be covered to prevent evaporation.

Looking at the frequent occurrence of disastrous fires in cotton mills, especially those of Lancashire, I wrote some years ago to an eminent local paper, strongly recommending that the owners of cotton mills should set tanks on the tops of their mills, and furnish these with supplies of ammoniacal gas liquor, to be always ready for service. I gave, too, a short description of the best way of arranging pipes to these different rooms. The gas liquor itself, by careful manipulation, may be pumped off very fairly clear, and free from tarry matter. Of course, besides to fires in cotton mills and tar distilleries, this mode of fire extinction might be all but universally applied. It may, indeed, be readily imagined how a jet of the liquor thrown into a burning room would act, the space being so inclosed, when the action is so powerful in a comparatively open space, and with such a refractory substance as burning pitch. It would seem, also, that the extinguishing power in the liquor would be even further called forth by a more intensely hot fire, for here, in all probability, the ammonia would be decomposed, and the generated nitrogen and hydrogen gases would help to swell the volume of non-supportive gas, displacing the air, and so choking out the combustion.—*Chemical News.*

SOFT COAL.

FOR years no one supposed that a lump of soft coal, dug from its mine or bed in the earth, possessed any other purpose than that of fuel. It was next found that it would afford a gas which was combustible. Chemical analysis proved it to be carbureted hydrogen. In process of time mechanical and chemical ingenuity devised a mode of manufacturing this gas, and applying it to the lighting of buildings and cities on a large scale. In doing this, other products of distillation were developed, until, step by step, the following ingredients are extracted from it:

1. An excellent oil to supply lighthouses, equal to the best sperm oil, at lower cost.
2. Benzole—a light sort of ethereal fluid, which evaporates easily, and, combined with vapor or moist air, is used for the purpose of portable gas lamps, so called.
3. Naphtha—a heavy fluid, useful to dissolve gutta percha, India rubber, etc.
4. An excellent oil for lubricating purposes.
5. Asphaltum, which is a black solid substance, used in making varnishes, covering roofs, and covering vaults.
6. Paraffine—a white crystalline substance, resembling white wax, which can be made into beautiful wax candles; it melts at a temperature of 110°, and affords an excellent light.

All these substances are now made from soft coal.

RATIONAL UTILIZATION OF THE WATER FROM FULLING MILLS.

By A. GAWALOVSKI.

IN the great centers of the trade the waste waters of fulling mills are utilized in two distinct manners—either by extracting in a single operation all the organic substances which they contain, and using the settlings and precipitates thus obtained for the manufacture of lighting gas; or by separating the fatty acids and isolating them either by mechanical means such as presses, or by chemical agents (gasoline, kerosene, essence of petroleum, sulphuret of carbon, etc.).

The first method is founded on the precipitation of the fatty acids in the state of calcareous soaps; the combinations formed by the fatty acids with the lime, alumina, and oxide of iron separated from the mordants employed in dyeing, and which are particularly soluble in soap water, act upon the waste waters and clarify them.

The main object here in view is the purification of the waters, the extraction of the fatty acids being merely an accessory operation. According to Stahlschmidt's system the calcareous soap after its separation is decomposed by means of hydrochloric acid, and the grease is finally extracted by means of sulphuret of carbon.

The direct separation of the fatty acids is more remunerative, and has formed the subject of many investigations. Nevertheless the question cannot be regarded as finally settled.

According to the author's analyses, the waste waters contain:

	Per cent.
Saponified fatty acids	0·973
Alkalies	0·656
Iron and alumina	0·217
Organic matter (not fibrous)	0·141

According to the most ancient process sulphuric or hydrochloric acid was added to these waters; after the lapse of half an hour to an hour and a half, a black muddy mass rose to the surface, and amounted to about 24½ per cent. of the water employed.

The residual liquid still contains:

	per cent.
Free fatty acids	0·074
Alkalics combined with mineral acids	8·857
Lime, alumina, etc	0·108
Extractive organic matters	0·364

After having been carefully drained, the mass is gently heated in cloth bags, when more water flows off, along with fatty acids, extracted by the aid of pressure. These fatty acids are of a deep brown color, and contain:

Fatty acids (partly free and partly in the form of soaps of iron and cotton)	98·34
Emulsed water	0·46
Mineral residue (iron, sand, lime, etc.)	0·43
Organic matter	0·77

The residue in the bags consists of:

Fixed fatty acids	21·34
Do. moist, with traces of volatile bodies	43·50
Mineral matter	4·88
Organic matter	30·28
	100·00

The fatty matter contained in this residue cannot be extracted mechanically, but by means of solvents. It is, however, better to utilize them for the manufacture of lighting gas and for heating boilers. An attempt has been made to convert into soap, but the operation requires much water, and yields a product in little request.

The fat expressed from the deposit as above mentioned contains often mineral acids, and is therefore rarely fit for oiling wools or for lubricating purposes. The author has found in a sample of this kind used for lubricating cards 0·108 per cent. of mineral acids, in consequence of which the cards were rapidly attacked. It may, on the other hand, be used for the manufacture of soap, as is often done in practice. The grease is used either pure or mixed with palm, cocoa, or olive oil, or with tallow.

The author has examined some of these soaps, and has found the following components:

	No 1.	No. 2.	No. 3.	No. 4.
Water	22·94	27·57	30·69	31·73
Fatty acid	61·57	58·90	61·64	60·35
Soda	15·21	13·40	7·54	7·82
Organic matter, etc.	0·37	0·23	0·13	0·11

The samples 1 and 2 were strongly alkaline soaps, used in the manufacture of heavy cloths, and not requiring the addition of soda in fulling; while the samples 3 and 4 were neutral soaps used in fulling fine goods.

The waste waters of fulling mills contain a considerable quantity of fibers in suspension. Thus in a waste water at Jägerndorf Gawalovski found:

Water	95·50
Fibers	1·70
Sediment	2·80
	100·00

If we suppose that 11.000 gallons of water are daily consumed, this represents a daily loss of 187 pounds of wool. It is very true that such wool is necessarily short, and very bad to spin, and that it is often mixed with cotton. But we cannot but consider as simple waste, a product the quantity of which in a single establishment may reach the yearly weight of 66,000 lbs., and which at any rate might be used in the manufacture of felts, or in those of ammonia and Prussian blue.

The method of utilizing these waste waters has been variously modified, but without coming to any definite results.

The author has studied for about five years a process, the object of which is to separate from the deposit all its impurities, mineral salts, gelatinous matters, etc., and thus to increase its percentage of fatty acids. The grease obtained is almost pure and exempt from mineral acids, which permits it to be employed in the manufacture of toilet soaps, in preparing the oil baths for Turkey-red dyeing, etc.—*Moniteur de la Teinture and Deutsches Wollengewerbe.*

REDUCTIVE ACTION OF MILK SUGAR UPON ALKALINE SOLUTION OF COPPER.

By H. RODEWALD and B. TOLLENS.

THE authors point out that, while chemists are agreed on the reductive power of dextrose, there is great discrepancy concerning lactose, 1 mol. of which is considered to represent various quantities of copper oxide, ranging from 6? to 8 atoms. They find that the exact quantity required is 7·47 atoms of copper to 1 mol. of milk sugar, =6·700 mg. milk sugar to 1 c.c. of Fehling's liquor. They recommend that this reagent should not be prepared in quantities beforehand. Sixty grms. of the best caustic soda and 173 grms. of recrystallized tartrate of potash and soda are dissolved in a half liter of water, while 34·639 grms. pure copper sulphate are dissolved separately in another half liter. Equal volumes of these two liquids are mixed when wanted.

NATURE OF POISON OF YELLOW FEVER, AND ITS PREVENTION.*

By Dr. H. D. SCHMIDT, New Orleans, La.

YELLOW FEVER is a disease, running a regular course, and accordingly depending, like small-pox, scarlet fever, measles, and other kindred diseases, upon a specific poison of animal origin, being a product of the diseased human organism itself. This assertion is proved by the characteristic feature it presents in common with all other specific diseases, consisting in the immunity from a second attack, imparted to the individual once affected; and this immunity from a second attack, which most strikingly distinguishes specific diseases from all others, must be constantly kept in view, in studying the nature of the peculiar poison producing yellow fever. In connection with this prominent feature of the disease, I may be permitted to mention that, from what I have learned, my friend Professor J. Dickson Bruns has, in his lectures to medical classes, always laid particular stress upon this feature of yellow fever, for the purpose of proving its contagious nature.

The precise nature of the particular pathological changes which impart to the organism this immunity from the effects of the poison afterward, we are at present unable to determine. Perhaps the most popular view is that the poison alters the constitution of the blood in such a manner, and so permanently, as to be unable to make any further impression upon it afterward. Equally difficult is it to say whether the changes are wrought upon the colored blood-corpuscles in particular, or upon the plasmatic portion, the liquor sanguinis; furnishing the nutriment to the organs and tissues of the body. Judging from the unusually rapid disturbance in the nutrition of the organism, manifested by the phenomenon of fatty infiltration and degeneration in a number of organs, and also by nervous disorders, the liquid portion of the blood would appear to be first affected; while, on the other hand, the facility with which the blood corpuscles seem to part with their hæmoglobin, as my observations have shown, indicates that these bodies must likewise be affected. For a number of years I have, in relation to the difference existing in the impression of the poison upon the blood in miasmatic and in yellow fever, been in the habit of illustrating it in a somewhat homely manner by assuming that in the former the impression of the poison is only functional in character, and in consequence evanescent; while in the latter it is organic in its nature, and therefore permanent, and insuring immunity from a second attack. But, though it appears to me very reasonable that the immunity should depend upon certain permanent changes, wrought by the poison upon the blood, I cannot forbear to presume that similar changes may at the same time take place in the nervous tissues, rendering them in the same manner unimpressible for another attack.

Besides the immunity from a second attack, characterizing yellow fever as a disease produced by a specific poison, the clinical symptoms, and, moreover, the anatomical changes observed after death, also speak for its peculiar and specific character. But, as a specific disease connot be produced in the human body by a general cause, it is obvious that the specific poison producing it must be derived from another human body affected by the same poison, and that this poison must necessarily be a product of the organism of the affected individual. We may, therefore, presume that a general cause cannot produce a specific disease.

To illustrate this proposition, we may take, for example, a number of persons of different age, sex, and constitution, exposed to a sudden and severe change of temperature by leaving, insufficiently clothed, a warm and overcrowded room, and entering the cold night air of the street. It will be found, in this case, that the effects produced by a general cause—the cold air—upon these persons, may differ in each individual; for, while in the one they may manifest themselves in the form of a simple coryza, they may result in the others in bronchitis, pneumonia, rheumatism, diarrhea, or other affections, implicating the one or the other organ of the body. Each of these different affections, at first local in its nature, may, if extending, involve the nervous system sufficiently to give rise to the phenomenon of fever. Here we see how one and the same cause produces different effects, according to the peculiar diathesis or constitution of the affected individual. But, if now, on the other hand, a number of these persons, some time after they have recovered from their various affections, are introduced into a room, the air of which is tainted with the exhalations of a number of small-pox patients, while the rest of them are taken to a room infected with the exhalations of yellow patients, it will be found that the first party will surely be affected with small pox, and the other with yellow fever, unless they had been affected with these diseases at a previous time. Each person will, to the exclusion of every other disease, become affected with that particular disease of which he has inhaled the specific poison, emanating from the affected individuals. In every contagious disease, therefore, the infectious poison first proceeds from the diseased organism itself, and, in the same manner as we have seen in putrefactive septicæmia, increases in intensity with each individual through whom it passes.

From this explanation, it will be seen that yellow fever, when it first appears, never attacks a number of persons, isolated from each other, at one and the same time, but always, like every other contagious disease, starts from single centers, represented by the infected individuals. And the only manner in which it spreads is either directly from individual to neighboring individual, or indirectly, when the poison, emanating from the affected individual and adhering to clothes and other objects, is carried to a distant place and inhaled by another person. A number of centers may arise in different localities of a city, which, increasing in dimensions, may at last meet. In this manner alone the infectious poison is able to extend over a large city, like New Orleans; and the surest proof of this assertion is offered by the slow march in which the disease walked over our city during the last epidemic. An infectious poison, on the contrary, being contained and distributed throughout the air of a city or locality, either in the form of living organisms, or in the form of vegetable or animal effluvia arising from decomposing matter, will necessarily affect every person susceptible to infection nearly at the same time, and, in consequence, rapidly extend over a city or district. For this reason, the theory of a *contagium vivum*, or of the miasmatic or effluvial origin of yellow fever, is entirely inconsistent with the contagious character of the disease. But, notwithstanding these existing circumstances, there are still a number of physicians who, while they recognize the contagiousness of yellow fever, entertain at the same time the incongruous idea that the cause of the disease is represented by minute living organisms, and, for the sole reason, as they say, that this is the only theory upon which they can understand the multiplication of the poison, as nothing could multiply unless it were endowed with life.

I shall try to explain this subject satisfactorily by a suitable comparison. In every organized body, whether plant or animal, we meet with an albuminous substance which is regarded as the basis of life, and is generally known by the name of "protoplasm." This protoplasm, as we all know, forms the basis of all tissues performing a vital function, whether consisting of cells or fibers; and, though it is not an organized body, it nevertheless represents living matter and is endowed with the power of appropriating other matter unlike itself, and of imparting to it its own properties; or, in other words, of converting this matter into protoplasm. Now, it is upon this principle that all organs and tissues of the body not only rejuvenate themselves but also increase in size. Thus, the protoplasm of a muscular fiber appropriates new matter and converts it into muscular fiber; the protoplasm of a ganglion cell imparts its properties to the same new matter furnished by the plasma of the blood and converts it into nervous tissue, etc. The new matter is in all cases the same, but it is the protoplasm of each particular tissue which represents the converting agency.

The specific poison, having entered the circulation, and being diffused throughout the blood, imparts, like the protoplasm, its own properties to this fluid, giving rise to all the pathological phenomena characterizing the disease. Still, it must be remembered that the degree of intensity of the disease depends entirely upon the quantity of the poison taken up by the blood. If the quantity is sufficiently large to affect all the blood circulating through the organism, the disturbance of nutrition would be so great as to interfere at once with all the vital functions, and the case would prove fatal very rapidly. We may, therefore, presume that, with some rare exceptions, in all other cases of yellow fever the poison affects only a smaller or larger portion of the blood, leaving to this fluid sufficient integrity to nourish the organs and tissues at least for some days, thus affording an opportunity to the organism to rid itself of the poison. This, however, can be accomplished in no other way than by means of the action of secreting, or, as we may say in this case, excreting cells, the same organs through which the organism rids itself of all other foreign or wasted matter. Thus the poison, already increased in quantity by that portion of blood to which it imparted its own properties, is absorbed from this fluid by the cells of different glandular organs during its passage through the capillaries surrounding the individual minute glands. And, in passing through these cells, it again increases in quantity by imparting its noxious properties to their natural secretion, together with which it it is finally eliminated from the organism. In yellow fever, therefore, as in all other contagious diseases, it is in the secretions, especially those of the glandular cells of the skin and of the respiratory passages, that the noxious poison is contained, and with which it is removed from the system.

In some contagious diseases, the poison, before leaving the body, gives rise to certain pathological processes in the epidermic cells of the skin, manifested by divers exanthematous eruptions which, in some instances, as in small-pox or the cow pox, proceeds to the formation of vesicles and pustules containing the poison in liquid form. Nevertheless, even in these affections, a portion of the poison leaves the body in a gaseous form, possessing the same virulent properties as the liquid contained in the vesicles and pustules. In yellow fever the poison emanates from the body of the affected individual only in a gaseous form, in which it may be communicated to another individual, or may adhere to surrounding objects, as clothes, bedding, furniture, etc. In the latter case, however, it may be supposed that, while adhering to these objects, it undergoes a certain condensation, in which state it is transported to distant places. But, as soon as acted upon by a certain degree of moisture and heat, it again assumes the gaseous form, in which it may then be communicated to other persons. But, as the intensity of the attack of the disease is perhaps in most cases proportionate to the amount of poison taken up by the affected person, it is not necessary that the first case to which the poison is conveyed by the medium of these objects shall terminate fatally. On the contrary, a number of persons may successively become affected without a fatal termination. But, as the yellow fever poison, like the purulent poison of septicæmia, increases in intensity with each individual through whom it passes, death will eventually be the result. This fact will explain the phenomenon of the considerable interval of time which has frequently been observed to intervene between the first fatal cases of yellow fever; cases appearing in different localities of a city and between which no connection can be traced, for the reason that it is only the fatal cases which in the beginning of an epidemic attract attention.

Much has been said and written about certain meteorological conditions required for the activity and propagation of the yellow fever poison, such as a certain amount of moisture in the air, together with a continued tropical heat, a deficiency of ozone, etc. Though nothing positive concerning these conditions has thus far been ascertained, I entertain no doubt that these suppositions are to a certain extent founded upon truth; for, the fact that yellow fever has assumed the dimensions of extensive epidemics only at shorter or longer periods, while, on the other hand, it has frequently existed in the form of so-called minor epidemics, or been even represented only by a few sporadic cases, shows that these atmospheric conditions are not always present. Neither does the noxious poison, after its absorption into the blood, manifest its activity in the same degree in different persons, a phenomenon to be explained by the different degree of susceptibility which these persons possess to the effects of the poison, depending probably upon the particular state of their constitutions at the time when it is brought in contact with the latter. Thus we frequently meet with persons who have been exposed in many different ways to the action of the poison during several severe epidemics, without ever being affected, but who, after the lapse of many years, are finally attacked by the disease. Nothing is known of the nature of this singular immunity, whether it is owing to a peculiar constitution of the blood or of the nervous system.

Although the customary assertion that native-born or long acclimated persons enjoy immunity from an attack of yellow fever has now been sufficiently proved as unfounded, it can not be denied that, in most instances, acclimatization greatly ameliorates the severity of the attack. This is most strikingly observed in unacclimated persons, especially in those who have lived in northern latitudes, beyond the yellow fever zone, who were generally the first victims of the disease. And it has become a general rule, corroborated by all writers, that the nearer a person has been born and lived to the North Pole, the more liable he is to be attacked by the disease, and the smaller his chances are for recovery. This fact evidently shows not only that there exists a difference in the constitution of the blood of these different persons, but, moreover, that the air of the yellow fever zone must in some essential points differ from that of northern climes, enabling it to exert a certain unknown influence upon the constitution of the blood of those persons who breathe it, adapting it, so to speak, to the yellow fever poison. And to this circumstance it is mainly due that the rate of mortality during the last epidemic has been proportionately much larger in Memphis, Vicksburg, Holly Springs, and numerous other places, than in New Orleans, or even in other parts of southern Louisiana.

Although the facts already mentioned suffice to prove the animal origin of the poison producing yellow fever, and the contagiousness of the disease, I shall, in corroboration of this assertion, adduce some additional evidence. In discussing in the preceding pages the application of the germ-theory to infectious diseases, I have already shown its futility, unless the actual presence of the bacteria in the blood of the diseased individual is proved in every case occurring; for even a few exceptions will show that the presence of these organisms is not essential to the disease. Now, in yellow fever no bacteria, or any other living organisms, are found in the blood of the patients during any stage of the disease. As I have stated in my paper on the pathology of yellow fever, published in the February number of the New York *Medical Journal*, 1879, I examined at the Charity Hospital, during the last epidemic, very carefully, the blood of fifteen living patients in the different stages of the disease, without detecting even a single bacterium, or any other foreign body in this fluid. Neither did I observe any of these organisms in the blood when examined shortly after death; it is only when decomposition commences that they make their appearance. But not only is the blood, but the other tissues also are found free from anything living, provided they are guarded from decomposition. These facts require no further comment, as they show most forcibly the impossibility of applying the germ theory in any way to yellow fever. There is, however, another fact corroborating my assertion that the noxious poison of this disease represents a vitiated secretion of the diseased body itself. It relates to the peculiar odor associated with certain contagious diseases, such as small-pox, etc., emanating especially from the secretions and exudations of the skin. In small-pox this odor is sufficiently strong and characteristic to enable the physician, in some cases, to diagnosticate the disease by the olfactory sense alone, and before the eruption has appeared. In yellow fever, also, there is a peculiar odor associated with the exhalations of the patient. This odor is so peculiar as to have always been observed, and also described by a number of writers; some of these even regard it as a pathognomonic symptom of the disease. Emanating from the body of the patient, the odor may be regarded as a manifestation of the contagious principle in its gaseous state. And judging from its strength, as it emanates from only a single patient in a badly-ventilated sick room, it is easy to form an idea of the quantity of the poisonous principle to which it is due; and, furthermore, of the facility with which it may adhere to the clothes of visitors to be carried to distant places, or how easily it may communicate itself through the open windows to neighboring houses and people.

The peculiar odor associated with the exhalations of yellow fever patients, I noticed very particularly while I was engaged in examining the blood of living patients during the last epidemic, and when, in order to obtain the specimen of blood, I was obliged to approach and remain for some time in very close contact with the patient. And it was during these examinations that I became affected with the disease myself, though I had escaped it in former epidemics—an attack, however, from which I soon recovered. But it is not only the living body from which this odor is perceived to emanate; on the contrary, the cadaver also, as long as it is warm, is characterized by a peculiar odor, slightly differing from the former. Here, however, it is especially perceived when the cavities of the body are opened, or when the muscles are deeply cut into or laid bare.

The question, whether yellow fever may spontaneously arise among us, has always been agitated among medical men, and, owing to the existing diversity of opinion regarding the nature of the disease itself, has never been satisfactorily answered; and unless physicians will base their opinions only upon solid, observed facts, instead of unobserved fancy, it never will be correctly answered. As far as I am able to judge, yellow fever is almost always communicated from person to person in the manner already explained; and reasoning from this fact, it is obvious that, in order to arise anew after having been once extinguished, it must be freshly imported. But, besides this, it is also possible that some of the poison, adhering to clothes and other objects during the winter, in a state of inactivity, may resume its activity as soon as the particular conditions required are present, and thus give rise to new cases of the disease. The possibility of such an origin is proved by a number of authenticated cases of scarlatina, variola, and other contagious diseases, which arose in this manner. But, in considering that the disease must have arisen spontaneously at some previous period, however remote, it is only reasonable to admit also the possibility of a spontaneous origin at the present time. However, if the disease ever arises spontaneously in this manner, it can only be confined within the limits of a small locality, and in the presence of all the elements, necessary to a spontaneous origin of the disorder in the human organism, from which eventually the specific poison emanates. Typhus fever, though in most cases communicated from person to person, has spontaneously arisen in this manner, when a large number of persons, insufficiently clothed and nourished, have been crowded into a small room without ventilation. In such a case the exhalations emanating from the bodies of the various individuals, already vitiated by the broken-down constitutions, are inhaled again, and, together with other causes arising from the sufferings and wants of these individuals, such as a continued exposure to cold, etc., give rise to that peculiar condition of the blood characterizing this fatal disease. Yellow fever may sometimes arise under similar circumstances, though, unlike typhus, only in the presence of a high temperature, combined with moisture and other atmospheric conditions. Such an origin, however, appears to me not very probable, though its possibility cannot be disputed.

Regarding the decline and final disappearance of an epidemic of yellow fever, it is very generally believed by physicians, as well as by the people, that the noxious poison is destroyed by frost. The apparent truth of this assertion, however, is rendered very doubtful, not only by the consideration of the fact that at New Orleans the epidemic has almost always nearly disappeared when the first frost occurred, but, furthermore, by the new cases arising frequently long after the appearance of this meteorological phenome-

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non. Thus, in the epidemic of 1867, I paid the first visit to my last case of yellow fever on the tenth day of December, a case which, though not proving fatal, was nevertheless a severe one, accompanied with jaundice and extensive hemorrhages from the lungs, the patient being an unacclimated foreigner. The same circumstances obtained, as far as I remember, during the last epidemic at Memphis and other places situated more northerly than New Orleans, where the disease continued to prevail for some time after the appearance of the frost. Thus the epidemic always reaches its maximum height a considerable time before frost appears; and there is no doubt that its decline is mainly owing to the decrease of the number of those persons susceptible to the action of the yellow fever poison, and also to the gradual disappearance of those unknown meteorological conditions of our atmosphere which, when present, favor the evaporation of the poison from the bodies of the affected individuals, and its inhalation by other persons; while, at the same time, the systems of those persons who thus far escaped the disease are reinvigorated with the approach of cooler weather in the month of October. These are, I suppose, the main causes to which the decline of the disease may be attributed, though the germ-theorists firmly believe that the frost kills the germs, representing, as they suppose, the cause of yellow fever.

In regarding the nature of the cause of yellow fever, such as has been expressed and demonstrated in the preceding pages, there will be no difficulty met with in determining the proper means of prevention, especially if an agent could be found possessing the property of effectually destroying the activity of the yellow fever poison, adhering to the clothes and other effects of passengers, or to merchandise brought in ships to our shores from distant ports infected with the disease, but without damage to these objects themselves. Without such an agent it is impossible to establish a quarantine both efficient and, at the same time, not interfering seriously with the interests of commerce. It is, therefore, of the greatest importance that to this subject particularly the attention of scientific men should be directed. In recent times very numerous investigations and experiments have been made by the scientific men of Europe for the purpose of discovering a disinfecting agent possessing the properties above mentioned; and, accordingly, many substances have been tested, the most efficient of which, however, are impracticable for the purposes of a quarantine. As it is impossible to give an account of the details of these experiments in this place without enlarging upon the subject, I will only mention that the process of oxidation is generally looked upon as the surest means of disinfection, and that, in consequence, free ventilation is in all cases the first law to be observed. Thus the exposure of infected articles to the fresh air alone may be sufficient to cause the decomposition of the adhering noxious poison. More efficient, however, will be the artificial application of pure oxygen, especially in its electrical state, that of ozone. Dry heat, also, is very effectual, and has been used for a number of years in European hospitals for the purpose of disinfecting the clothes of patients affected with contagious diseases. In fine, the poison must either be decomposed by oxidation, or be rendered harmless by diffusion in the higher strata of the atmosphere, where it will undergo a still more certain decomposition by the influence of the ozone upon it. Sunlight, also, is a most efficient agent for the destruction of animal poison.

But if, notwithstanding an efficient, well-regulated quarantine, yellow fever should again make its appearance in our city, the most important sanitary measure would certainly consist in a perfect solution of the first cases from the surrounding community, a policy which in every European country is practiced with the greatest care and severity, and which is the only sure way to be pursued for preventing a contagious disease from assuming the form of an epidemic. If thus the poisonous principle is confined to a limited locality it must eventually become destroyed by the proper means, or even by the action of the air alone.

In concluding this article, I cannot forbear to point once more to the absolute necessity of first studying more systematically the pathology of yellow fever before we can hope to keep this disease successfully in check—that is, confined to limited localities; and, furthermore, to renounce all empty speculations regarding the nature of its cause; but, on the contrary, to look the enemy in the face, in regarding it as a contagious disease of animal origin, such as I have demonstrated in this essay. For if, on the other hand, the cause producing yellow fever really consisted of minute living organisms, existing in large numbers in the air we breathe, and capable of multiplying indefinitely, as has been asserted by some writers, it must be obvious that any quarantine, however strict and well regulated it might be, must prove a failure. And, moreover, as these organisms would be inhaled by every person alike, and indefinitely multiply within the blood, there would not remain the slightest hope for any person ever to recover from an attack of yellow fever.

THE ELECTRIC LIGHT IN LARGE CITIES.

AT the recent session of the British Parliamentary Committee, Mr. William Henry Preece, assistant engineer and electrician to the Post-office, was a witness. In reply to the chairman, he said he had devoted much attention to the progress of electric lighting. The application of this light to the Post-office was one of serious importance—first, on economical grounds, and secondly, from a hygienic point of view. The Post-office paid about £7,000 a-year for gas, so that any means of reducing the expense was a matter of importance to them; and again, in their large telegraphic galleries they had at times 1,000 people all through the night, during which a great part of the work was done, owing to the press messages. Five million cubic feet of air were vitiated, of which one million were by the operators. The other four millions were vitiated by the gas, and it was of importance to be able to remove those four millions. He had examined the various systems of electric light in London and Paris, and seen everything that was possible in connection with the subject.

The Chairman: What are the conditions you think should be attained for illuminating the great telegraph rooms?

Mr. Preece replied that three conditions were necessary: (1) Any electric light should be of the brilliancy of at least 1,000 candles, as the structure of their rooms was such that such a light would enable them to be illuminated as efficiently as they now were. (2) The light should be absolutely steady, for in carrying out the work—reading dots and dashes on thin strips of paper—it was essential that the eyes should not be wearied by an unsteady light. (3) The light must last all night long without requiring attention. Their lights in winter sometimes were burning for eighteen hours, and therefore any electric light must at least last that time. He had not yet found any lamp that had combined these three conditions. The obstacles to this did not, however, consist in the currents, for in the dynamo machine perfection was almost reached; and the Siemens machine reproduced in the form of electric currents 90 per cent of the capital energy thrown into it. The difficulty was in the lamps. The Serrin lamp gave the requisite brilliancy, but not the durability; the Wallace-Farmer would burn for a week, but did not give a steady light; the Wer'armann was perhaps the steadiest lamp, but it did not me't the said essentials. He had made experiments as to the subdivision of currents, and he found that if a series of lamps were joined upon one current the intensity of each lamp diminished with the number inserted. As to the cost, he thought each horse-power economically applied ought to give a light equivalent equal to 1,250 candles. The lights on the Embankment were produced by a Gramme alternating machine, which was practically four machines using 90-horse power. Each of these machines ought to give 6,250 candles of light, but they were joined up in series, and in each wire there was a series of five candles. The square of these was 25, and if 6,250 were divided by 25 the result was 250, which ought to be the light of candles not obscured by globes. The globes diminish the light by 40 per cent., so that the light on the Embankment ought to be equal to 150, and photometrical experiments agreed with that figure. The electrical light was only economical when one machine was used to produce a single light, otherwise there was waste.

The Chairman: What is your opinion as to the appliance of the electric light for general street lighting?

Mr. Preece said he did not think it was applicable, because if a long street had an electric light at one end sufficiently powerful to illuminate the extreme end of the street, then at the nearest end there must be an enormous light, for as the light diminishes with the square of distance the loss at one end must be largely in excess of the light at the other. From that point of view he had written a paper in which he brought out a result so extraordinary that he almost hesitated to accept it—that a street of 1,000 ft. in length with 40 gas-lamps of 15-candle power would be better lighted than with one electric light of 6,000-candle power. But in symmetrical spaces, such as squares, then one single light would have the same effect as a largely distributed light, and would also be economical.

The Chairman: The point upon which I am specially anxious to examine you upon is whether there is any danger to be apprehended from the general use of wires to convey electric light with regard to the telegraph wires?

Mr. Preece: That is a point to which we have paid a great deal of attention; and as soon as we found so many corporations applying for power to lay down pipe wires, I investigated this question to see how far such wires would interfere with our working of wires. In the first place it was necessary to ascertain the relative power of these different currents, and I found that the strength of the light currents was 3,000 times that of the telegraph currents. I placed telegraph wires alongside light wires, and watched their effect with a telephone. The wires were 6 ft. apart, and we found a very powerful effect; and the inference from my experiments was, that we could not place our wires within 6 ft. of the lighting wires, so that any wires conveying illuminating powers ought to be at least that distance apart, unless they are crossing each other or are only of short length. As Mr. Cooke said, a return wire would prevent induction, but that would necessitate doubling our system.

In reply to Lord Lindsay, Mr. Preece admitted the condition that an electric light should be equivalent to 1,000 candles within a small limit; but he had placed it thus low because lamps capable of that power existed, though they did not meet his other conditions. But there had been a great advance in the direction of steadiness, and there were indications of ultimate success in that direction. According to theory, if there were two lamps at different points in a large hall, one-fourth of the central light would be obtained, but practically the result was less than one-fourth. He did not think the telephone would be an instrument of the future and be largely adopted by the public; for although it had been largely adopted in America, we had not the same necessity for it, for we had a superabundance of messengers for all purposes, which the Americans had not. His calculation that forty 15-candle lights would be better than one electric light of 6,000, was so startling that he was inclined to think there must be some mistake.

Lord Lindsay: I am rather inclined to agree with you in that calculation.

Mr. Preece said further that the Post-office never used return wires, and he could not tell what they would cost, but their adoption would mean the doubling of their present system, which had been laid down at a cost of £10,000.

By Sir U. K. Shuttleworth: One great disadvantage of the electric light would be that the shadows would be all in one direction.

By Mr. Moore: There would be no objection to the light and the telegraph wire being more than 6 ft. apart.

By Sir D. Wedderburn: He laid stress on the hygienic advantages of the electric light as against gas, because of the vitiation of the air by the latter. Where one person would vitiate 3,000 cubic feet of air one gas burner would vitiate 9,400 ft. The persons employed in the Post-office galleries did not suffer, because every possible precaution was taken, and they had a splendid system of ventilation.

By Mr. Henry: The light used at the Albert Hall and the British Museum would not serve for the Post-office, for it would not burn long enough nor sufficiently steady. The same wire could not be used for lighting and for telegraphing, because of the difference in strength, the strength of the lighting current being infinitely greater than the telegraphing current.

By Sir D. Wedderburn: At first the telegraph wires were placed within 6 in., of each other, but as the apparatus improved, and the work increased, the distance was enlarged, till now they were 18 in. apart, and even more than that for long circuits, as from London to Holyhead. As to why the Post-office did not put their wires underground, as the Germans did, that would involve three or four times the cost of the present system, and would diminish the transmitting power of the wires to some extent.

By Lord Lindsay: Last year the Post-office transmitted messages from London to Ireland by the Wheatstone automatic instrument at the rate of sixty-one words a minute, but now the rate was 150 a minute, and next year it would be 200; but if light wires were placed near these wires, the speed would be reduced probably to sixty-one.

By Mr. Stanhope: The Post-office had not adopted the magnetic machine; they used only batteries for producing currents.

EDISON'S ELECTRIC CANDLES.

IN a recent English patent, by T. A. Edison, the special feature of the invention relates to the candle for the diffusion of the electric light. Mr. Edison's candle is in the form of a slightly tapering hollow cylinder, divided vertically, except at the upper end. By this arrangement he claims that uniformity and complete incandescence are secured as the electric current passed up one side and down the other. At the base of the cylinder, which is enlarged for the purpose, the electric conductors are connected. A thermal circuit regulator, which has been described in an earlier patent recently published, is attached at the bottom of the candle, and is so arranged that, if the current becomes excessive, the regulator becomes heated, and, consequently, expands so as to bring a movable spring into contact with the adjusting screw of the illuminating apparatus, and thereby diverts the current and lessens its action in the light.

Mr. Edison specifies the materials of composition and method of manufacturing the light-giving substance, the essential condition of which is necessarily its power to resist fusion by the heat developed in the passage of the current. He says:

"Metals or oxides of metals are made use of which produce in a comparatively large candle sufficient resistance to render the whole incandescent. Finely divided metal or particles of metal having a high melting point are caused to adhere by earthy materials, such as magnesium, or zircon oxides, or magnetic oxide of iron, or other substances that are with difficulty fused. The oxides of metals may be obtained by chemical precipitation or otherwise, and the candle is moulded either in a dry or moist condition by pressure. The fine particles of metal may be platinum, iridium, rhuthinum, or other metal."

The patentee then details the mode of manufacture:

"In cases where these oxides or earthy materials are mixed with the fine metallic particles, such particles are thoroughly and uniformly mixed into a mass before it is moulded, and hence such particles are kept separate by the earthy material, and cannot fuse or run together, and the metallic oxides themselves are electric conductors to a greater or less extent, and hence may in some instances be used without the particles of metal. In all cases the substances are moulded and pressed into shape under powerful pressure, and if the particles do not adhere together sufficiently, sugar, tar, silica, or similar substances may be used in moulding the candle into shape, and these substances may be volatilized by the heat. The metallic particles are rendered highly incandescent, and the earthy substances and oxides are also rendered luminous by the passage of the electric current."

Finally, the patentee declares that his invention "is not limited to any particular form in which the candle may be moulded or otherwise shaped, but it relates to the materials employed in such candles, whereby the mass is adapted to the passage of the electric current, and it is rendered highly luminous or incandescent by such current."

In the statement of claims appended to the specification Mr. Edison sets forth as follows:

"What is claimed as the invention secured by these letters patent is that a candle or light-giving body for electric lights, in which particles of metal or metallic oxides are moulded into a mass, is adapted to the passage of the electric current, and made luminous by the same, substantially as set forth."

THERMIC AND GALVANOMETRIC LAWS OF THE ELECTRIC SPARK PRODUCED IN GASES.

By E. VILLARI.

THE heat developed in gases by an electric spark is proportional to the quantity of electricity which produces it. The galvanometric deviations produced by the discharge of Leyden jars are proportional to the quantities of electricity condensed. The galvanometric deviations produced by one and the same charge of the condensers are constant and independent of the length of the spark produced at any given point of the circuit. If one and the same quantity of electricity amassed in any condenser is discharged through a metallic circuit, interrupted so as to give rise to a spark, the quantity of electricity brought into play in the discharge is constant and independent of the length of the spark. The quantity of heat developed in any gas by the electric spark increases in proportion to its length, whence the temperature of the spark at its different points is independent of its length, and the electric resistance of the gases is proportional to the thickness of the gaseous layer traversed by the discharge. When the charge producing the spark remains the same the quantity of heat developed by this spark is independent of the surface of the condenser. In fine, the thermic and galvanometric deviations produced, the former by the spark and the latter by the discharge of a condenser, are proportional to the quantity of electricity which produces them, and at the same time to the length of their active circuits (this name being applied either to the length of the spark or to the length of the galvanometric wire).

THE CURIOUS ASTRONOMICAL PHENOMENON.

To the Editor of the Scientific American:

Seeing that you have honored me with a reprint of my letter to the New York *Tribune*, dated April 13, in which I gave a superficial description of a comet-like object moving through space from northwest to northeast, and being daily questioned by astronomers as to correct and more details of data, I take pleasure, now that the matter seems to become one of general interest, in giving a more minute statement, and at the same time vindicate myself—being accused by some of the "learned professional astronomers" for not having published my discovery through the proper scientific channel and in the proper manner. I am as yet in the infancy of astronomical researches and totally inexperienced in the discoveries of "wonders," such as "volcanoes in action on the moon's surface," "second companion to Polaris," "satellite to Venus," or "a third satellite to Mars," and a thousand other wonderful discoveries that are yearly reported to the Smithsonian Institute or the Naval Observatory, and thrown into the waste basket. I have never made any discoveries, never intend to. I have an utter detestation for people who speculate in scientific matters, who stir up the world with dispatches of their discoveries on one day, and the next become the laughing stock of an intelligent community.

I did not think that the above phenomenon was anything but of a meteoric nature, and coupled with this belief, which I at present adhere to, it would have been ridiculous for me or any one else to have made a great outcry. The publication of my letter to the *Tribune*, and the message sent by me

to Prof. Hall, were both urged by a personal friend, whom I called into the observatory to see the object, otherwise it would this day only be known to my immediate friends; the coolness with which my dispatch was received at the Washington Observatory (as it was no little trouble for me to send it so late at night, living over a mile from the telegraph office) has compelled me to regret any publicity on my part. (The presumption of astronomers that I had discovered Brorsen's comet ought to have been abandoned immediately, from the fact that the motion of Brorsen's comet was a little over a degree per day, whereas this object moved with a rapidity of over 2 minutes in R. A. to one minute of time, and on its path passed the above named comet by about 4° at 9 h. 10 m. local time, having then already moved 78 m. of R. A. since I first saw it.) There is one fact, however, which reconciles me to it, and that is the fact that my object was also seen by Mr. J. Spencer Devoe, of Manhattanville, N.Y., who published a letter to that effect a few days afterward.

I am indebted to my esteemed friend, Henry M. Parkhurst, for the anxiety and attention which he has devoted to this matter, and sincerely hope that, as a result of our correspondence, he may throw some light on the subject.

Below I give the data of R. A. and Decl. at the different times of observation:

April 12, mean local time.

At	8 h. 40 m.,	P. M.	R. A.,	2 h. 34 m.	D. N.,	37°	
"	9 h. 10 m.,	"	"	3 h. 46 m.	"	37°	
"	9 h. 35 m.,	"	"	5 h. 4 m.	"	37°	
"	10 h. 30 m.,	"	"	7 h. 8 m.	"	37°	6′
"	11 h. 30 m.,	"	"	—	"	37°	28′

April 13.

"	2 h. 10 m.,	A.M.	R.A.,	15 h. 30 m.	"	37°	30′

From the above it will be seen that its motion was anything but uniform. In the first 30 m. of time, it moved 72 m. in R. A., being 2·4 m. of R. A. in one minute of time; in the following 25 m. of time, it moved 78 m. of R. A., being 3·1 m. R. A. in one minute of time; in the next 55 m. of time, it moved 124 m. of R. A., being 2·6 m. R. A. in one minute of time; and, lastly, in 220 m. of time, it moved 502 m. of R. A., being 2·3 m. R. A. in one minute of time. Average motion per minute of mean time, 2·49 m. of R. A.

At 11 h. 30 m. I neglected in the excitement to take positions of R. A., also at 2 h. 10 m. on the morning of April 13; but, it being then almost due north of *a* Serpentis, I approximated its R. A. to be about 15 h. 30 m.

I have been dilatory about publishing the above statement, on account of Mr. Devoe's promise to give further position, as he luckily saw the same object and watched it until moonrise. I hope that gentleman will speedily send to the editor of the SCIENTIFIC AMERICAN a postscript to my statement.

Hoping that the above may be granted publication in your valued journal, I am, etc., HENRY HARRISON.

Jersey City, N. J., May 20, 1879.

CURIOUS ASTRONOMICAL OBJECT OBSERVED BY MR. HENRY HARRISON, APRIL 13TH, 1879.

LEVERRIER.

WE present herewith a representation of a bust of the celebrated French astronomer, Leverrier, executed by the sculptor, Préault.

Urbain Jean Joseph Leverrier was born in St. Lô, March 11, 1811, and died in 1877. After a course of study at the Colleges of St. Lô and Louis le Grand, he graduated at the Polytechnic School. Obtaining a place in the Tobacco Bureau, and finding that that occupation required some knowledge of chemistry, he pursued the latter science at leisure, and, in 1837, published two memoirs on the combination of phosphorus with oxygen and hydrogen. Mathematics, however, became the principal object of his study, and from his proficiency therein he soon obtained a minor appointment in the Polytechnic School. From this time on he directed his studies toward the elucidation of the highest problems in speculative astronomy, investigating especially the irregularities manifested in the course of the heavenly bodies.

Two memoirs on this subject, presented to the French Academy of Sciences in 1839, attracted the attention of Arago, who, becoming his friend, induced him to study closely the orbit of Mercury and its perturbations. In 1844 he presented two important papers to the Academy on comets, and the importance of these contributions to science caused him to be elected to the astronomical section of the Academy of Sciences. The success that had attended his calculations of the course of Mercury induced him to revise the still more imperfect tables of Uranus. After a thorough study of the subject, he became convinced that the movements of the latter planet could not be explained by the attraction of any known bodies, and he, therefore, sought further for the cause of its perturbations. At length, on the first of June, 1846, he indicated to the Academy of Sciences within ten degrees the place where a new planet might be seen January 1st, 1847. This was, in fact, seen by the German astronomer, Galle, four months before the time indicated, viz., on the 23d of September, 1846. Leverrier had erred, but only by a difference of two degrees.

JEAN JOSEPH LEVERRIER.

This discovery caused an immense sensation, and Leverrier received abundant honor. Most of the learned societies of Europe inscribed his name on their lists; the King of Denmark sent him the Order of Dannebrog; Salvandy, the Minister of Public Instruction in France, had his bust erected in public with great ceremony; Arago declared that the new planet should be called Leverrier; a chair of mathematical astronomy was created for him in the Faculty of Sciences; the Royal Society of England sent him the Copley gold medal; and the Grand Duke of Tuscany a splendidly bound copy of the works of Galileo. The planet bore the name of Leverrier for a short time only, that of Neptune being subsequently bestowed on it.

In 1848 Leverrier made some ineffectual efforts to become distinguished as a democratic leader, but it was not till 1849 that he was elected to the Legislative Assembly from La Manche. Modifying his liberal views, and taking his place among the counter-revolutionary members, he devoted himself to questions of public instruction and laws relative to scientific discovery. When a division into parties took place in the Assembly, he joined the Imperialists. After the *coup d'etat* in 1851, he was appointed Senator, and subsequently became Inspector General of Public Instruction. In 1849–50 he read to the Academy of Sciences the result of his new investigations into the movements of the planets, and in 1853 he presented the same body tables of the sun's rotation, with the complete system of the small planets situated between Mars and Jupiter.

On the death of Arago, in 1853, Leverrier succeeded to the title and authority of Director of the Observatory. In 1859 he communicated to the Academy a movement of the perihelion of Mercury, which could only be accounted for by supposing another planet, or a series of small bodies, moving between it and the sun. This brought out Dr. Lescarbault's assertion of his discovery of a planet in 1859, and which he had named Vulcan. But subsequent researches have failed to establish the existence of such a planet. In 1870 Leverrier withdrew from the office of Director of the Observatory, owing to differences that had arisen between himself and other eminent astronomers. Delaunay became his successor, but he having lost his life by drowning, in 1872, Leverrier was reappointed.

During the Franco-German war Leverrier offered his services to the Government of National Defense, which employed him in perfecting a system of optical telegraphy that he had invented, and which was intended to render communication possible with Rouen or Orleans, by using the light of the sun reflected from a mirror, and astronomical telescopes sweeping the horizon in a given direction. The illustrious astronomer succeeded after a few months in devising a complete system, but the Prussian invasion had driven the French forces to such a distance, that the curve of the earth opposed an insurmountable barrier to the working of the signals. This system, after the establishment of peace, was communicated to the Academy of Sciences, and it has been used recently with great success by the British army in Afghanistan and Central Africa.

Not long since a public subscription was opened at Paris for the purpose of obtaining funds to erect a statue to the great astronomer, and many illustrious persons contributed, but the committee in charge have just met with an unexpected difficulty. The authorities declared that Leverrier should have no statue, because Arago had had none, and they, therefore, refused the authorization asked for. These municipal councilors, however, have quite recently unknowingly rendered a more precious homage to their illustrious countryman, whose image they have banished, than the erection of a statue would have been. At one of its later sittings the council voted the sixteen thousand dollars necessary to establish a line of wires for the purpose of obtaining a uniformity of time in the public clocks by means of electricity.

The designing of such a system was one of the creations of the latter days of the great French astronomer, and is of greater importance than is generally supposed. As well known, the hour is transmitted from the termini of railways to all intermediate stations, and from the latter it is sent to the public buildings and principal hotels of the place. Now, thanks to electricity, the Paris Observatory will transmit time simultaneously over the whole national domain.

QUINCE CULTURE.

THE quince first attracted notice in the city of Cydon in Crete or Candia, hence its botanical name, *Cydonia*. There are several varieties, some of which are only used as ornamental shrubs, and some are only valued as stocks for dwarfing pears. The principal varieties of value for eating are the apple-shaped, embracing the well known orange quince; the pear-shaped, inferior in quality to the others; the Portugal, a shy bearer of excellent quality; and a large seedling variety much boasted of in these days of great things.

My experience thus far is in favor of the orange quince. It is hardy, grows rapidly, and bears abundantly. I have now twenty-two trees that were propagated from cuttings only five years ago, that yielded three bushels of very fine fruit last season. The largest of them weighed ten ounces, and thirty-three filled a peck measure heaped as long as they would lie on. The best of the trees bore to maturity forty-seven, and would have had more but for a vigorous thinning out when quite small. The price of quinces for a number of years has been from $2 to $4 a bushel, and scarce at that. If they were cultivated and cared for with the attention of other fruits, I think they would be found among the most profitable crops.

Propagation by cuttings is easy, and comparatively sure. The cuttings should be cut from the trees before the buds begin to start in the spring. I prefer February or March. Take well ripened wood of one and two years' growth, the older being the choice of the two, and insert in the soil about a foot deep to secure them against drought, leaving three or four inches above the surface for the development of buds. Sticks of half an inch diameter will be found to send out the most vigorous shoots. I had from a cutting, one-third of an inch in diameter, a growth in 1878, of three branches measuring respectively 3 feet, 4 feet, and 4 feet 8 inches. About 2 feet is a fair average for the growth the first year. The growth onward will be according to the cultivation. They begin to bear, with me, the third year from the cutting, and yield a good crop the 5th year. I have now (March 1st) a medium sized quince, that grew last year on a tree but three years old. The base of the tree above the collar is not larger than my thumb. It blossomed full, and I allowed this one quince to remain till the wind of a severe autumnal gale broke the little twig that held it, when, as a curiosity, it was hung up by a window in the kitchen, where it still hangs, entirely sound.

My method of culture is to set the standard trees in quincunx rows, about eight or ten feet apart, by which method I gain one row in nine over the method of setting in squares, and yet keep the desired distance. In preparing the ground, I dig a hole a foot and a half deep, about three feet across, and farther if any roots are long. I then fill in a few inches of rich earth, on which I set the young tree, covering the roots carefully with more rich soil, and near the surface put a liberal mulch for the double purpose of preventing drought and enriching the ground. Managed in this way my trees uniformly live, and make a vigorous growth. A little salt is grateful to the quince tree; and adds to its vigor.—*W. W. Meech, Vineland, N. J., in N. E. Farmer.*

COMPRESSED FLOUR.

THE *U. S. Miller* tells the world that flour subjected to a hydraulic pressure of 360 tons can be reduced in volume more than 25 per cent., and yet retain all the qualities it had previously to its violent treatment. Some of the pressed flour was put into hermetically tight tins. At the same time other flour manufactured from the same wheat, but not compressed, was also sealed up. About three months after several boxes containing both kinds of flour were opened and examined. The pressed was pronounced to be the best. Twelve months after this another examination took place, and with the same result. The two kinds were kneaded into loaves and baked. The pressed flour made the best bread. In another year after the boxes were opened and examined, and while the loose flour showed mouldiness, the pressed was sweet, and retained all its qualities. Made into bread the same difference was observable.

THE NEW CHURCH OF THE ORATORY.

We have already published four designs submitted in competition for the above church, and this week we are enabled to give to our readers a series of plates illustrating the selected design, "D. O. M." (to which the first prize was awarded), by Mr. Herbert A. Gribble, Bayswater. It will be seen, on referring to the plan, that the one great object aimed at was simplicity of arrangement, but providing for no less than nine chapels, one alterino and baptistry, and twenty confessionals. It was the author's ambition to make the chapels as large as the site would permit, considering it indispensable to so numerous a community and congregation.

The aisle chapels measure 30 ft. square, and are connected together by openings sufficiently large for processional purposes. There is an ample supply of exterior doorways, and for the purposes of ingress the doors under each campanile will be used, while those at the bottom of nave are mainly intended for egress. The small door and staircase shown next the east transept are for the use of the choir, and lead to the organ chamber over Calvary Chapel, also to the cloak room, etc., in basement. And, again in the west transept, provision is made for two corretti having communication with the first and second floors of the priest's residence adjoining. The materials proposed to be used are —for the exterior, yellow brick and Portland stone dressings, and for the interior the large columns and pilasters will be in Devon red marble, with pedestals in grey ditto; the smaller columns will be executed in Genoa green, and the walls of such material as will lend itself for future decoration. The sacristy is a very spacious apartment, measuring 53x30 ft., well lighted by a continuous lantern carried on a groined cove supported by caryatides, and will be fitted up with walnut paneling and presses.—*Building News.*

SUGGESTIONS IN ARCHITECTURE.—THE NEW CHURCH OF THE ORATORY, S. KENSINGTON.

MODELING IN CLAY.

By EDWARD A. SPRING.

BY carefully observing the following rules, and studying their principles, almost any one can indulge in the fascinating occupation of modeling in clay, with fair prospect of most gratifying results. No process could be simpler. A lady, seventy years of age, has been known who began clay modeling, and, without any instruction, made several good medallion portraits; at the other extreme there are Indian children who make lifelike animal figures from the clay found in natural beds.

Notwithstanding these facts, all beginners must expect to encounter difficulties, and must regard repeated failure only as beneficial experience. Palissy himself says, in his "Art de Terre," that his greatest advance was by mere chance, after much fruitless experimenting, and it may be accepted as a fact that he learned most by his failures. Above everything else, the modeler should endeavor to start right. These five maxims meet the difficulties of most beginners, and, if practiced, will save much time.

MATERIALS FOR MODELING.

I. Clay, such as potters use, free from grit and wet with water.

II. Modeling wax (used for very small work). Beeswax, colored with Indian red or other dry color, melted together with a little rosin, Venetian turpentine, Burgundy pitch, and tallow. (Price, $1.50 to $2.00 a pound.)

III. Plaster. ("Casting plaster" costs less, and is stronger than the "superfine.")

TOOLS FOR MODELING.

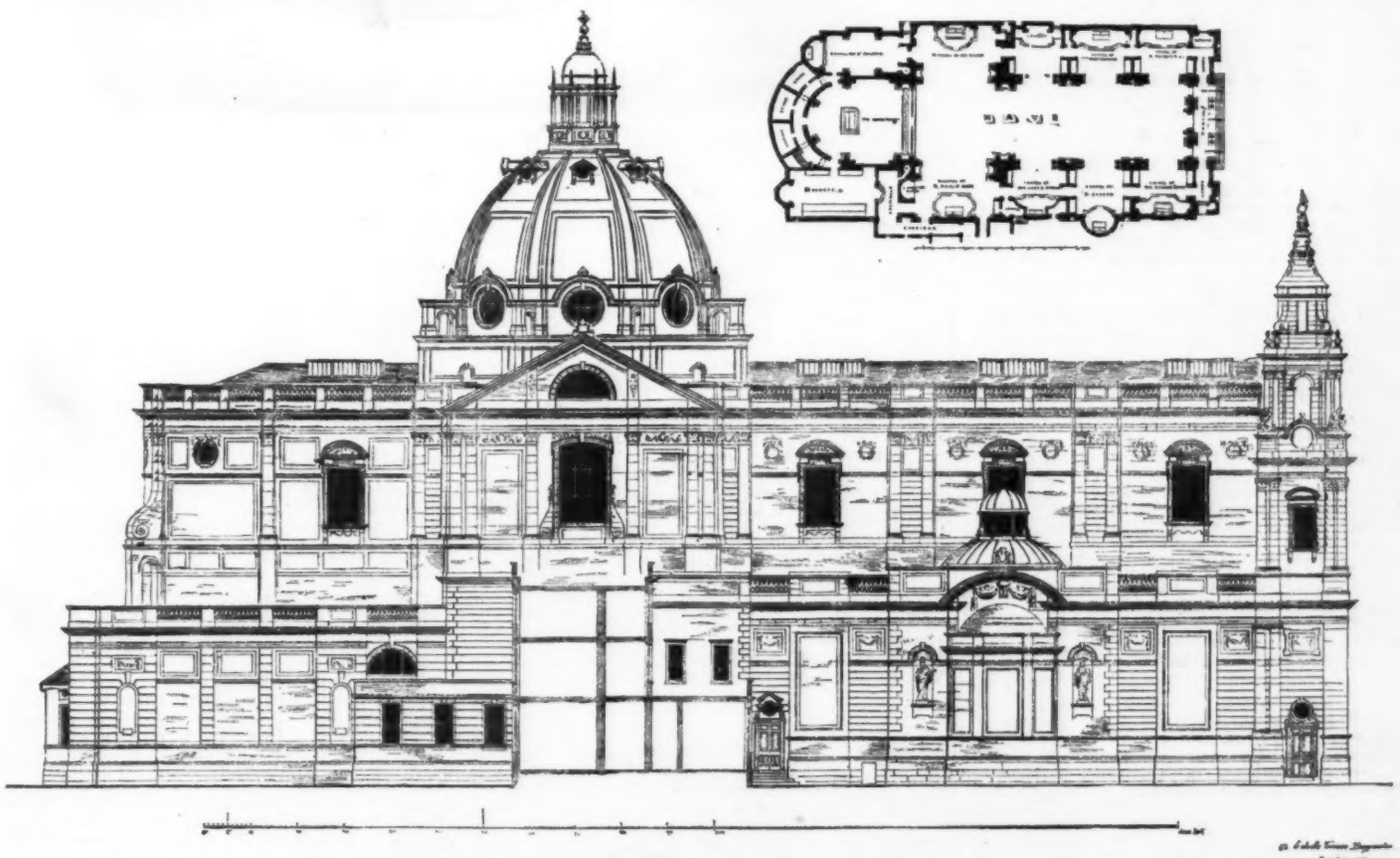
I. The fingers and thumbs first and best.

II. Modeling sticks (boxwood, red cedar, pine). A knife, spoon, crochet needle, or hair pin.

III. Wire, size of horse hair, to cut clay.

IV. Small sponge.

V. Place to wash the hands, with abundance of water.



THE NEW CHURCH OF THE ORATORY.

PRACTICAL HINTS FOR MODELING.

I. How to use the clay.

See that in adding clay to clay both surfaces are smooth. Rugged surfaces of moist clay will not stick together.

Leave no air confined, and the clay work will stand firmly, and, if terra cotta clay is used, can be baked in a kiln.

II. How to see the work.

A strong light is wanted, from above the level of the eye. Turn an object in the hand, or the hand itself, and you will see that the slightest roughness of surface is clearly visible only between the lightest and the darkest places, *i. e.*, in the "half light." Therefore, in finishing especially, the delicate modeling must be done by frequently turning the clay, or moving the light, so as to work on the "half light."

III. How to be neat in clay work.

Whenever clay begins to dry on the hands, wash them with a few rapid sweeps of a wet sponge, and rinse them well in several waters. This will keep the hands soft. Do not dry them on a dusty towel. If clay dries upon the hands it falls at every movement and gets tracked about. It also scatters on the work and destroys the finish.

Avoid touching the clay with wet hands, as that makes mud. The finger tips are sometimes used dry and sometimes wet. A modeler generally keeps a damp sponge, to be touched by the tips of the fingers and the tools.

IV. What kind of tools to use.

In modeling, there can be only three kinds of surface to make, viz., plane, convex, concave, and their combinations. Any tool that will produce a given result with the fewest motions of the hand is the best to use. Clay could be shaped by simply pricking and scratching it with a point. But, as such a point would be the least effective and slowest kind of tool, we may conclude that, to accomplish the most at each stroke, the largest tool should be used. It is the knowledge of these details of manipulation that saves the learner from discouragement or loss of time.

V. The use of plaster moulds.

If there is a wish to produce the same or nearly the same form in clay several times over, it would be convenient to have a tool so shaped that, by simply pressing, the form could be repeated. Such a tool is found in a plaster mould. The resources for accurate scientific study and comparison, and the various practical ways of utilizing this method of work, it will doubtless take years to develop.

To make the mould.

(1) Surround the area for each mould or piece of mould with a "fence" of clay or other material. (2) Spray it with a solution of soap. (3) Mix plaster, and fill the space so prepared, and in half an hour the mould can be used.

PRICES OF MATERIALS AND TOOLS

I.	Clay. Three cents a pound, at retail. A tub with forty pounds of prepared clay	$1 00
II.	Tools. A set of four wooden tools and fine wire.	1 00
	A "potter's sponge	10
III.	Plaster. $1.75 a barrel. Small box of plaster for moulds, etc	50
	Total	$2 60

N. B.—These are beginners' tools.

PRACTICAL MAXIMS FOR MODELERS.

I. Add smooth to smooth.
II. "The modeling is in the half light."—*Hunt.*
III. Be neat. Keep the hands free from dry clay. Do not work in mud.
IV. "Use the largest tool."—*Ward.*
V. "Make plaster moulds, when needed, to serve as modeling tools."—*Spring.*

—*Art Interchange.*

WINGS AND ARMS.

Translated from the "Scholastic."

[THIS is as literal a translation as could be made, consistently with the preservation of the meter and rhyme, of the first of those exquisite *Chansons Physiologiques* of the Abbé Tirebouchon, which have contributed so much to the education of the masses, by spreading among them scientific truths arrayed in the attractive garb of poesy.—*La Demande de Mme. Blaguedent.*]

I.

"How I wish," Mrs. Blodgen remarked, "I could fly!"
While pensively sipping her tea,
As she gazed at the flock of wild geese going by,
Whose pinions extended were cleaving the sky,
"How happy these creatures must be!"

II.

"If wings from my shoulder-blades only would sprout,
How gay through the clouds I would whirl,
Like those dear little Cupids that used to come out
On the letters St. Valentine's Day brought about,
Which I often received when a girl!"

III

The learned Professor, with elegant ease,
To her wishes then said in reply:
"To mar so poetic a dream will displease;
Yet the 'dear little Cupids' that poetry sees
Are monstrous in science's eye.

IV.

"Of the fowl that has furnished our modest repast
Some osseous fragments remain,
And on those of the wing, if your eye you will cast,
Your own observation will aid you as fast
As the words that I use to explain:

V.

"The *humerus*, *ulna*, and *radius* here
In beautiful order you find;
And now in your skeleton arm will appear
The very same bones, as is perfectly clear,
In the very same order combined.

VI.

"So you plainly perceive that an arm is a wing,
Though somewhat deficient in feather;
And no vertebrate animal, though he were king
(I hope to your mind the conclusion I bring),
Can have wing and arm both together.

LA TUFFOLINA.—THE NEW STATUE BY O. TABACCHI, NAPLES.

VII.

"'Tis true supplementary limbs may be found
In butterflies, spiders, and such;
But you wouldn't, I think, if your reason is sound,
Wish to give up your back-bone, and flounder around
With *articulata*—not much!"

VIII.

But here Mrs. Blodgen got mad, and declared
That such language was awful indeed,
And that he could repeat it again if he dared.
Here she lifted a broom. The Professor prepared
To flee from the table, and never more cared
Conversation in that way to lead.

LA TUFFOLINA.

THIS is the title of a new piece of artistic sculpture, shown in our engraving, lately completed and exhibited in Naples by the author, O. Tabacchi. It was immediately purchased by the King of Italy; and it had so many admirers during the short period of its exhibition that, it is said, the fortunate sculptor will have difficulty to fill all the orders he has received for copies. Every bath house in the country is to have one, where it will serve in the warm season as a model of attitude for all lady patrons of such establishments.

The figure could hardly be rendered more beautiful, elegant, or sympathetic, nor more graceful in attitude, representing as it does the bather in the act of diving into the water as if she were an expert swimmer. The works of the above well-known artist are everywhere celebrated for their unusual merit.

THE TREE CREEPER.

CERTHIA FAMILIARIS.

THE leading trait of the brown creeper is its extraordinary industry—the "incomparable assiduity," as it has been well styled, with which it works for a living. Like all good workers, the creeper makes no fuss about it, but just sticks to it. So quietly, yet with so much celerity, does it go about its business that it scarcely seems to be at work, but rather to be rambling in an aimless way about the trunks of trees, or at most only caring to see how fast it can scramble up to the top. During all this time, however, the bird is on the alert in the search for insects, which it extracts from their lurking places with such dexterity that its progress is scarcely arrested for a moment; and the number of these minute creatures yearly destroyed is simply incalculable. The creeper is strongly attached to the trunks of large trees, being seldom seen foraging on even the larger branches; and it has a great fancy for traveling upward. These two traits combined result in its marked habit of beginning its curious search for insects near the bottom of a tree, and ascending with jerks in a straight or spiral line to the top. Then, if it likes the tree, and thinks it a good place to stay awhile longer in, the bird launches itself into the air, and drops down on wing, to begin another ascent, in preference to scrambling down again, as a woodpecker or nuthatch would do. The easy, gliding motion with which it climbs has deceived one writer into stating that the creeper does not hop along like a woodpecker; but, in fact, the movement is exactly the same in both cases. One of the English writers (Barrington, Zool., 2d ser., p. 3998) describes, however, something peculiar in the position of the feet during the act of climbing: These, he says, are not held parallel with each other, and near together, under the belly, but widely straddled, and thrown so far forward as to form with the end of the tail a surprisingly broad-based isosceles triangle. So nimble is the bird, and such a sly way has it of eluding observation by turning in the opposite direction to that in which a person moves to look after it, thus continually interposing the trunk of the tree in the line of vision, that it is no wonder the way it holds its feet long remained unascertained. Many things conspire to screen the queer little bird from any but the most patient and closest scrutiny during its ordinary vocations; and so nearly do its colors correspond with the tints of the bark that it is likely to be overlooked altogether. But its habits are so methodical and undeviating that when one has learned them there is no difficulty.—*Coues' "Birds of the Colorado."*